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Skeptical Inquirer

THE MAGAZINE FOR SCIENCE AND REASON

Volume 27, No. 3 • May/June 2003

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Hypnosis, Airplanes, and Strongly Held Beliefs

PMS and Menstrual Disorder Myths

A Patently False Patent Myth

Wired to the Kitchen Sink

Mediumship Claims: Schwartz and Hyman Respond

REVIEWS of
- **Pinker**
- **Wolfram**
- **Shermer**

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EDITOR'S NOTE

Good Luck in Reading Our Cover Article

Good or bad luck can transform peoples' lives. So it's understandable that much of human culture is devoted to encouraging the former and discouraging the latter. To the degree that such efforts are based on magical rituals and bizarre behaviors, the lack of success poses no surprises. But, in fact, people can and do make their own luck. And some of this can be studied and demonstrated scientifically.

Psychologist Richard Wiseman's article "The Luck Factor" in this issue, based on his new book of the same title, describes his 10-year Luck Project, a scientific investigation into the concept of luck. The goal was to find why some people "seem to live charmed lives full of lucky breaks and chance encounters, while other experience one disaster after another." His research revealed four basic principles by which people tend to generate their own good fortune. (One: "Lucky" people are skilled at creating and noticing chance opportunities.) He carried out experiments to test the differences between such people and their less lucky brethren. Ever constructive, he even devised a "luck school," a series of experiments examining whether people's luck can be enhanced by getting them to think and behave in a more positive way. The answer is yes.

Wiseman heads the Perrott-Warrick Research Unit in the Department of Psychology at the University of Hertfordshire, north of London, where he has established an international reputation for his research into deception and parapsychology and for his efforts on behalf of the public understanding of science. A CSICOP Fellow, SKEPTICAL INQUIRER consulting editor, and former magician, he's an innovative, energetic researcher with a popular touch. A survey by *The Times Higher Education Supplement* found that he was the psychologist most frequently quoted by the British media. *The New Yorker,* writing about his widely publicized project last year to identify the world's funniest joke, called him "Britain's most recognizable psychologist." This is his sixth SI article.

* * *

We've expanded our reviews section in this issue to accommodate discussions of some important new books, plus a museum exhibit.

* * *

We note with sadness the death of Marcello Truzzi (see page 9). Marcello was the first editor of this journal. He edited the first two bi-annual issues, when it was called *The Zetetic;* I succeeded him in August 1977. We renamed it the *SKEPTICAL INQUIRER* but kept "The Zetetic" as a subtitle for several more issues. Marcello went on to publish his own informal journal, *The Zetetic Scholar,* at irregular intervals for some years thereafter. Always provocative, and with a sociologist's perspective, Marcello was as likely to find fault with skeptics as with paranormalists, but his goal always was to facilitate constructive debate between the two camps.

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NASA/European Space Agency's Solar Observatory Shows How to Make a UFO

WILLIAM THOMPSON

Of the many scientific satellites in space, the Solar and Heliospheric Observatory (SOHO) seems to have particularly drawn the attention of UFOlogists. SOHO is a joint project of the European Space Agency and NASA, launched in December 1995 to study the Sun. Daily images of the Sun in extreme-ultraviolet (EUV) wavelengths, and of the extended solar atmosphere (corona) in visible light, are made available to the general public through SOHO's Web page at http://soho.nascom.nasa.gov. However, almost since day one, there have been claims that these images show more than the solar features that SOHO was designed to study, and that there is a NASA conspiracy to hide these facts from the public. Occasionally these claims center around previously unknown planets in the solar system ("Planet X"), but the most common claim is of extraterrestrial crafts, i.e., UFOs. Recently, one group of UFO proponents, EUROSETI, decided to hold a conference on the subject, which was held at the National Space Center in Leicester, England.

Over the years, the SOHO team had become used to fielding questions from the public about UFOs in their images, but once the EUROSETI conference was announced, the team started receiving so many requests for information from both journalists and the general public that they decided to post a response. The SOHO team started with a purported UFO image that was published in an Australian newspaper reporting on the conference, and found the original SOHO image that it was based upon. In their article, they outline the series of steps needed to take an innocuous flaw produced by a cosmic ray hitting a few detector pixels, and process and stretch it until one ends up with something that looks like a UFO. Titled "How to Make Your Own UFO," and augmented with a detailed explanation of the various image artifacts that have led to UFO claims

in the past, the article is available at http://soho.nascom.nasa.gov/hotshots/2003_01_17/.

William Thompson is a solar astronomer, and SOHO/CDS co-investigator, working at the NASA Goddard Space Flight Center in Greenbelt, Maryland.

Psychics Fail Toronto Skeptics' $1,000 Challenge

In a direct public challenge to self-proclaimed psychics, Toronto-area skeptics put up $1,000 in prize money for anyone who could identify three common items in sealed boxes.

The $1,000 Psychic Challenge was held November 2, 2002, as part of the Psychic UnFair hosted by the Ontario Skeptics Society for Critical Inquiry (OSSCI). OSSCI organizes the UnFair each year to counter the psychic fairs and expositions held frequently in Toronto, as in other major North American cities, presenting mediums, astrologers, tea-leaf readers, aura painters, herb hawkers, and other paranormal practitioners to the public.

With so many local people professing psychic powers in the Toronto area, it might have seemed a sure thing that at least one of them would be able to claim the prize money.

However, at the end of the day, when the boxes were opened, it was found that no one had identified the three items.

Neither did anyone identify *two* of the three items in the correct boxes, which would have won them the $100 second prize. Nor did anyone identify even *one* item in the correct box.

The cardboard boxes were displayed in plain sight on a table behind a rope barrier, with a guard watching over them to ensure no one tried to touch them. People were able to view the boxes from six feet away and take their time to visualize the objects in them. They were told the objects were common items that could be easily identified by most adults or adolescents. Their answers were submitted in writing on a form. There was no charge for participating.

At the end of the UnFair, the boxes containing the mystery items were opened before a crowd of hopeful onlookers. The items were found to be a tangerine, a glove, and a roll of toilet paper.

A review of the submitted entries showed no one who had taken the challenge that day would win any of the skeptics' money.

No close matches—such as calling the tangerine an orange, for example—were found either, although these would have been honored as correct identifications, according to the published rules of the challenge.

The Psychic UnFair was held at the University of Toronto for the second year in a row, with an exhibit hall of exhibits and two presentation theatres. Exhibits included displays showing skeptical explanations of paranormal claims such as UFOs, the purported moon landing hoax, cryptozoology, and astrology. "Madame Liz" gave mind-reading demonstrations, after each of which she revealed to her amazed customers how the readings were done without paranormal powers. At another booth a skeptic explained how he successfully acted as an online psychic by using cold-reading and warm-reading techniques. Game booths tested skeptical skills and knowledge. Emily Rosa's experiment testing Therapeutic Touch was replicated at another booth.

Guest speakers throughout the day included psychologist and CSICOP fellow and Executive Council member James Alcock, who spoke on belief systems; magician and CSICOP fellow Henry Gordon, who related his lifetime of experiences debunking the paranormal; philosopher Fred Wilson, who has written and taught on science and pseudoscience; and biochemist Larry Moran, who has been prominent in defending evolution against creationism in Canada.

OSSCI is now working on developing a more extensive Psychic Challenge with a larger cash award—$10,000—as a greater incentive for psychics to take the test.

After the initial challenge, Toronto-area skeptics are confident their money will be safe even if they increase the stake tenfold.

—*Eric McMillan*

Eric McMillan is Chair of the Ontario Skeptics Society for Critical Inquiry.

Inorganic Oil: Much Ado about Nothing?

Geoscientists are cringing as news reports dredge up what they have long considered a preposterous assertion about the origin of oil: That none of the fossil fuels found on this planet come from fossils. The idea, heavily debated in Russia during the 1950s and 1960s, holds that the world's oil is not made of decomposed biological organisms; rather, it forms inorganically at near-mantle depths then migrates up to the crust.

The newest incarnation comes from J.F. Kenney, a self-proclaimed oil and gas driller from Houston, Texas, who worked with three Russian scientists, including Vladimir Kutcherov of the Russian State University of Oil and Gas. Their paper on inorganic hydrocarbon formation, published in the August 20, 2002, *Proceedings of the National Academy of Sciences* (*PNAS*), has generated coverage in *Nature*, *The Economist*, and *New Scientist* and led Kenney to an interview on National Public Radio (NPR). *PNAS* published the paper at the request of Academy member Howard Reiss, a chemical physicist at the University of California at Los Angeles. As per the *PNAS* guidelines for members communicating papers, Reiss obtained reviews of the paper from at least two referees from different institutions (not affiliated with the authors) and shepherded the report through revisions.

The paper examined thermodynamic arguments that say methane is the only organic hydrocarbon to exist within Earth's crust. The report also discussed the hypothesis that high pressures of 25 to 50 kilobar or more are needed for establishing natural petroleum hydrocarbon molecules. The authors also included a description of laboratory experiments in Moscow that created petroleum products from marble, water, and iron oxide under 50 kilobar of pressure and 1,500-degree Celsius temperatures.

But the news stories, Kenney says, are written on the premise that "I have 'developed a thermodynamic argument that demonstrates that the hydrocarbon molecules of natural petroleum cannot evolve spontaneously at the low pressures and temperatures of the near-surface crust of the Earth.' Such is absolute nonsense." Many geologists would agree. But, Kenney adds, "The fact that the hydrocarbon molecules which comprise natural petroleum cannot evolve spontaneously at the low pressures and temperatures of the near-surface crust of the Earth has been known by competent physicists, chemists, and chemical engineers for over a century. In my article, I only reviewed this knowledge briefly, using the efficient formalism of modern thermodynamics."

Kenney's slap in the face to the competence of modern geologists is not winning him any converts. Even astrophysicist Thomas Gold of Cornell University, who wrote two books on the subject of inorganic oil on Earth, is surprised by the media's response. "There is nothing new about any mix of hydrogen and carbon at pressures of 40 kilobar or so, and temperatures of greater than 800 degrees Celsius, forming oil."

Most commercial drilling occurs in sedimentary rock where source material temperatures range between 75 and 200 degrees Celsius. In the late 1980s and early 1990s, Gold spearheaded a project, which he says also involved Kenney, to illustrate the prospects of abiogenic oil and gas by drilling into crystalline rock in Sweden. But the granite did not yield an economically viable result.

Still, Kenney appears undaunted. During the interview on NPR, he said he found, while working with Kutcherov over the last ten years, inorganic oil and gas fields in the northern flank of the Dnepro-Donetsk basin in the Ukraine that are greater than the entire reserves in Alaska.

Kenney and his Russian colleagues' paper in *PNAS* is "an excellent and rigorous treatment of the theoretical and experimental aspects for abiotic hydrocarbon formation deep in the Earth," says organic geochemist Scott Imbus of Chevron-Texaco Corp. "Unfortunately, it has little or nothing to do with the origins of commercial fossil fuel deposits."

While geologists agree that crude oil can come from inorganic means, the majority of commercially recovered petroleum, they say, is organic. And they are frustrated with advocates of this alternative theory who dismiss evidence of a biological origin or interpret organics in crude oil as contaminants. Such an idea is anathema to the well-established understanding that biomarkers in petroleum are a result of living organisms transforming the complex molecules, dying, and then being subjected to burial processes that turn the biomarkers into petroleum products.

The idea of finding an abundance of crude oil ready for the tap at depths currently unreachable is tantalizing. But, says geochemist Alexei Milkov of the Deep Ocean Exploration Institute at the Woods Hole Oceanographic Institution and a graduate of Saint-Petersburg State University in Russia, "I've never met an industry geologist that uses abiogenic theory to find oil and gas fields, and that includes Russian industry geologists. These guys pay money for mistakes and can't afford using wrong theories to continue exploration." A key factor in deciding whether to put money in exploration

of a frontier basin is the potential quality and extension of source rock, Milkov adds. "This strategy apparently works for them so far."

That's a pity, says Roger Sassen, deputy director of Resource Geosciences, a geochemical and environmental research group of Texas A&M University.

"The potential that inorganic hydrocarbons, especially methane and a few other gases, might exist at enormous depth in the crust is an idea that could use a little more discussion. However, not from people who take theories to the point of absurdity," he says. "This is an idea that needs to be looked into at some point as we start running out of energy. But no one who is objective discusses the issue at this time."

—*Christina Reed*

Christina Reed is associate editor of Geotimes, *a publication of the American Geological Institute, where this story originally appeared. Published by permission.*

PBS *Evolution* Series Wins Science Writers Science-in-Society Award

The science documentary series *Evolution*, shown on public television in the U.S. in 2001, has been honored with the 2002 Science-in-Society award from the National Association of Science Writers (NASW).

In announcing the award January 8, 2003, NASW offered this description: "If there had been a science-in-society prize in Darwin's days, evolution would have been a real contender, and it obviously still is. In the television category, Richard Hutton, executive producer, WGBH/NOVA Science Unit, is honored for *Evolution* (broadcast September 24–27, 2001). Credit goes to the producers of this landmark series for recognizing the impact the science of evolution has had on society for well over 100 years. This captivating series took viewers from jungles to islands to classrooms to highlight the continuing scientific discoveries and political struggles over one of the most basic topics in all of science."

NASW holds the independent competition annually to honor outstanding investigative and interpretive reporting about the sciences and their impact on society for good or ill. The 67-year-old organization aims to recognize and encourage critical, probing works in six categories—newspaper, magazine, television, radio, Web, and book—without subsidy from any professional or commercial interest.

Penn & Teller Series Exposes Paranormal B.S. and Gains Audience

The raucous and irreverent magicians Penn Jillette and Teller are already well known for their in-your-face showmanship and implacable hatred of flim-flam. The duo has applied their signature talents to a new half-hour television program on the cable channel Showtime, titled "Penn & Teller: Bullshit." The show, which premiered on January 24, directs pointed criticisms onto a different paranormal or pseudoscientific topic each week, backed up by a merciless barrage of aspersions fired from Penn Jillette's bellowing howitzer of a mouth. This is the only program on U.S. television where viewers will hear mediums, creationists, and conspiracy theorists publicly—and repeatedly—called "assholes." I have seen two of the past nine episodes broadcast so far. Each outlines claims and skeptical arguments in a succinct, yet sufficient manner given the 30-minute format. "Bullshit" has already covered a broad range of topics, including alien abductions, the Apocalypse, alternative medicine, and *feng shui*.

The premier episode focused on the psychic medium business. There was a nice exposé of Rosemary Altea during a taped readings arranged by Showtime. Viewers got a clear picture of how she worked the small group of people present for readings prior to the taping in order to glean information for later use. They also learned how her publicist seeded the group with people whose biographies were already known to Altea in order to boost her on-camera success. CSICOP's Joe Nickell appeared on the show to discuss techniques of cold reading and the Barnum effect. CFI–West executive director Jim Underdown and *LA New Times* reporter Tony Ortega were also interviewed. They reported on observations they made during the undercover investigation of one of James Van Praagh's show. Underdown displayed an extensive and intimidating non-disclosure contract that Van Praagh's audience participants had to sign to keep them quiet.

The March 14 episode on creationism was also well done. Penn & Teller took viewers into the August 2002 hearings in Marietta, Georgia, where the Cobb County School Board heard arguments from creationist cranks and concerned citizens on the merits of "equal time" for Intelligent Design. The camera showed the live debate and protest, warts and all, from an agitated man in the lobby proclaiming, "If God is the God of creation, this whole place is doomed to hell," to otherwise normal parents and children who saw a "war between two different world views" and "two different kinds of people." The producers and writers did an excellent job of laying out the fundamental arguments and interviewing influential creationists such as Duane Gish of the Institute for Creation Research (ICR) and defenders of science education, such as Eugenie Scott of the National Center for Science Education.

By all accounts, "Bullshit" is doing quite well in the ratings. According to Showtime, ratings are up 14 percent overall in the show's time slot, and a whopping 40 percent in the male 35–49 demographic. Showtime's publicist for the series confirmed that the network was "pleased with the performance." Commercially successful *and* skeptical? If Penn & Teller actually pull this off (and they seem to be doing just that), then they will have a guaranteed spot in any magician's hall of fame, and any skeptic's hall of fame for that matter. Any demonstration to the media that skepticism sells will certainly help our cause.

—Kevin Christopher

Kevin Christopher is Public Relations Director for CSICOP.

Journal Editors Criticize MDMA Study as Nonscientific, Unethical

The editors of two journals that provide objective scientific investigations of controversial and largely untested medical and mental health practices have criticized proposed research using the drug MDMA (ecstasy) as a treatment for posttraumatic stress disorder. The study, "MDMA-Assisted Psychotherapy in the Treatment of Posttraumatic Stress Disorder," is awaiting final MDMA licensing approval from the Drug Enforcement Administration.

Proponents from the Multidisciplinary Association for Psychedelic Studies (MAPS) claim that it will prove that MDMA has strong potential as a psychotherapeutic drug. The president of MAPS, Rick Doblin, is a longtime proponent of recreational and spiritual use of both LSD and ecstasy. The protocol was developed in the Charleston, South Carolina, area with the assistance of MAPS. The proposed study subjects would be twenty victims of violent assault who have been given diagnoses of posttraumatic stress disorder.

MDMA has recently received national attention because of research published in the journal *Science*, implicating MDMA in damage to dopamine receptors in mammalian brains. Reductions in dopamine are associated with Parkinson's disease. Although the precise neurological effects of ecstasy remain controversial in the scientific community, there is a widespread consensus that this drug has the potential to do harm in at least certain cases.

Editors Scott Lilienfeld of the *Scientific Review of Mental Health Practice* and Wallace Sampson of the *Scientific Review of Alternative Medicine* (*SRAM*), based on detailed investigations by health advocate E. Patrick Curry, note that the research was approved by the Food and Drug Administration (FDA) in late 2001 and by the independent Western Institutional Review Board in July 2002. In light of reports of damaging effects of MDMA, the editors found the research to be potentially dangerous and possibly in violation of human subjects research ethical standards. They also note that evidence for effectiveness that would justify such research was lacking.

According to Lilienfeld, an associate professor of psychology at Emory University and president of the Society for a Science of Clinical Psychology, the study itself is scientifically questionable at best and meaningless at worst, because the treatment will not be compared with a meaningful and properly blinded control group consisting of either no therapy or a comparison treatment of known effectiveness.

"One of the most disturbing things about this study," Sampson said, "is that it appears to be the exclusive project of believers in psychedelic mysticism, and based on work of Dr. Stanislav Grof, an early LSD self-experimenter and psychedelic psychotherapist. After LSD and ecstasy use was declared illegal, Grof developed Holotropic Breathwork, a potentially dangerous form of severe hyperventilation, as a legal method of invoking hallucinations. Grof was a long-time fellow of the Esalen Institute, widely considered to be a birthplace of the New Age movement.

Both therapists involved in the research, principal investigator Michael Mithoefer and his wife, Ann Mithoefer, a psychiatric nurse, are trained practitioners of Grof's Holotropic Breathwork. The investigators' background, although not bearing directly on the methodological quality of the study, raises troubling questions concerning their capacity to conduct the research and to evaluate the data impartially without strong a priori allegiances.

Sampson and Lilienfeld question how such an experiment was approved by the FDA and an Institutional Review Board. Efforts by MAPS and the principal investigator, Dr. Mithoeffer, to win IRB support from the Medical University of South Carolina were rebuffed earlier this year. The project was approved within weeks when submitted to the independent IRB.

Sampson and Lilienfeld concur that comments in the popular press presenting this Charleston study as a possible "tie-breaker" in the debate over the effects of MDMA are misplaced. Lilienfeld states: "It is disturbing that scientifically flawed and potentially dangerous research like this could pass muster with the FDA, an IRB, and popular journalism."

New Center for Inquiry–Metro New York Office Opens

As of February 2003, the General Electric Building at Rockefeller Center is home to The Center for Inquiry-Metro New York office. Located at 30 Rockefeller Plaza, Suite 2829, the offices offer spectacular views, an extensive lending library, and space to serve the needs of skeptics and humanists from the surrounding areas.

Nearly 300 visitors attended a mid-March grand opening event at the offices, hosted by the permanent CFI–Metro New York staff as well as CSICOP chairman Paul Kurtz, *Free Inquiry* editor Tom Flynn, and SKEPTICAL INQUIRER managing editor Benjamin Radford. A statue of Prometheus, who took fire from the gods to give to Man, sits just outside the entrance.

Magnetotherapy—For Better Wine?

SKEPTICAL INQUIRER readers are familiar with the use of magnets for several proposed therapies, including pain relief by wearing magnet-insoled shoes and purification of water and fuels by magnetic treatment. But a new one has popped up on the skeptic's radar screen—the improvement of fine wines using magnets.

"Age your wine in minutes," proclaims the Web site that is promoting the Perfect Sommolier (www.cellarnot.com) for $49.95. The site even quotes Anthony Dias Blue, wine critic for *Bon Apétit* magazine, as saying, "The skeptic is convinced . . . Don't ask me how it works, but it works!" A short blurb in the December 2002 issue of *Bon Apétit* also trumpets this product.

What is the Perfect Sommolier? It's a set of magnets, one acting as a "magnetic base" for a wine bottle and the other a "magnetic stopper" to cork the bottle. According to the promotional information, you simply "wait 30 minutes. The wine will show softer tannins and rounder fruit, as if it had been aged for several years."

By what mechanism does this product work? No explanation is given, nor are any strict tests reported. Everything is promoted by testimonial. Thankfully, many serious wine drinkers have a healthy dose of skepticism—the only opinion they accept regarding the quality of a fine wine is their own! Unfortunately, unprepared wine appreciators may fall for the testimonials, which come from many one-named wine drinkers, distributors, and restaurant owners. The only other unambiguously identifiable people providing testimonials (besides our aforementioned journalist-critic) are Les Kincaid, listed as a "Master Sommelier" who does a radio show through CRN, a cable radio network, out of Las Vegas; and Fritz Sonnenschmidt, listed as the National Chair of the American Academy of Chefs. Mehopes Chef Sonnenschmidt is a better chef than skeptic. . . .

Some wines benefit from "breathing"—opening the bottle and letting some atmospheric oxygen dissolve in the wine. (Decanting also helps with that as well.) It would be like the difference between aerated and nonaerated water; nonaerated water tastes "flat" to us.

But a magnetic field? By what mechanism would the magnetic field change the tannin structure of a wine, or even decrease the acid level of the wine, as Chef Sonnenschmidt states? We get no clue. Given the inability of magnetotherapy supporters to find hard evidence for their previous claims, the ability of the Perfect Sommolier to "age" a wine may be equally impossible to substantiate.

My wine advice? Spend the $50 on a nice bottle of Veuve Clicquot champagne. Or a vintage port. And my friend Arnie at Markko Vineyards makes a very nice reserve cabernet. . . .

—David W. Ball

David W. Ball is an amateur wine snob and professor of chemistry at Cleveland State University.

Skeptical Gadfly Marcello Truzzi (1935–2003)

Longtime skeptic Marcello Truzzi died from cancer on February 2 at age 67. There are all too few skeptics in the world; Marcello was surely one of the most colorful and provocative: the skeptic's skeptic. He urged us not only to doubt claims that are ill-founded, but he was skeptical of skeptics who made up their minds prior to investigation. Truzzi befriended psychic Uri Geller and astrobiologist Michel Gauquelin—both of whom were meticulously investigated over the years by skeptical inquirers.

I am especially grateful that Marcello was involved in the early days of CSICOP. When I decided in 1975 to organize a conference on the growth of irrational cults and to create a committee to investigate them in 1976, I invited as many skeptics as I could locate. Sociologist Marcello Truzzi was on the list, for he was editing a small newsletter dealing with examinations of paranormal claims called *The Zetetic*, at Eastern Michigan University. He came from a family of circus performers and enlivened meetings in which he participated. He was so engaging that I invited him to co-chair our new Committee. He immediately accepted my invitation and edited The *Zetetic* in a new format under our auspices. But he resigned a year later (1977) after receiving a vote of no confidence from CSICOP's Executive Council. He wished our new Committee and its magazine to include both "believers" and "unbelievers," but the Council thought that there were literally hundreds of magazines and groups worldwide devoted to the pro-paranormal viewpoint, while almost none were interested in exploring a skeptical scientific agenda.

In any case, *The Zetetic* was soon afterwards retitled the SKEPTICAL INQUIRER under the editorship of Kendrick Frazier, and it has grown rapidly ever since. We were sorry to lose Marcello (who then created a Center for Scientific Anomalies Research in Ypsilanti, Michigan), and also to see that the new journal he founded, *The Zetetic Scholar*, was eventually discontinued. We should always be grateful to Marcello Truzzi as the skeptical gadfly.

—Paul Kurtz, Chairman, CSICOP□

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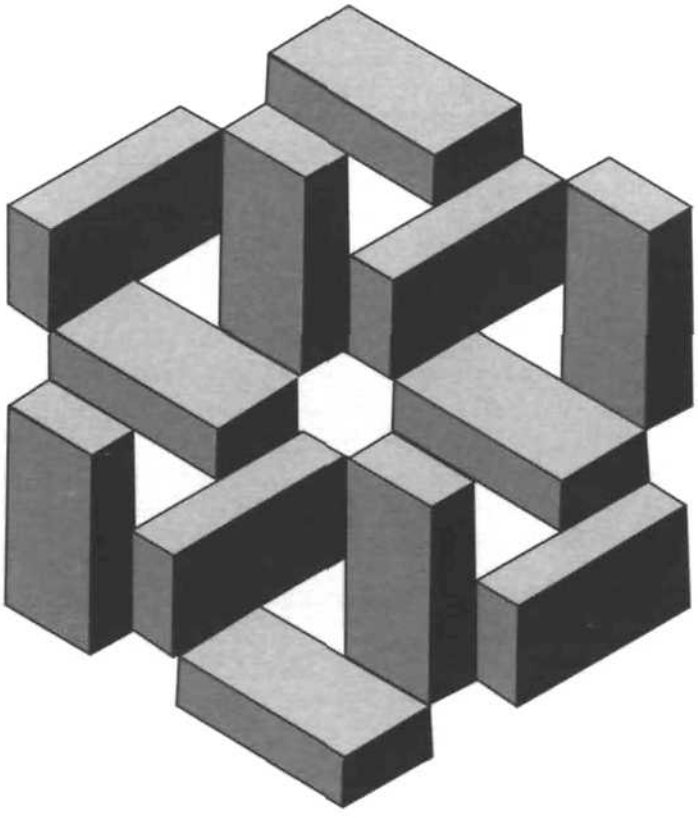
Main Session Faculty

Barry Beyerstein is associate professor of psychology and a member of the Brain Behavior Laboratory at Simon Fraser University. An expert in pseudoscience in healthcare, Professor Beyerstein is associate editor of *The Scientific Review of Alternative Medicine.*

Paul Kurtz is professor emeritus of philosophy at SUNY-Buffalo, and the founder and chairman of the Center for Inquiry. He is Editor-in-Chief of *Free Inquiry* magazine and is on the editorial board of *Skeptical Inquirer* magazine. Kurtz has published over 650 articles and reviews and authored or edited over 40 books, including *The Courage to Become* (Praeger/Greenwood, 1997), *Science and Religion* (Prometheus, 2003), and *Moral Problems in Contemporary Society* (Prentice-Hall, 1969).

Robert C. Solomon is Quincy Lee Centennial Professor of Philosophy and Business at the University of Texas at Austin, and a member of the Academy of Distinguished Teachers. A specialist in post-Kantian continental philosophy, he has also published extensively on ethics, business ethics, the emotions, and the history of philosophy.

Richard Wiseman heads the Perrott-Warrick Research Unit at the University of Hertfordshire, UK. His scientific research into unusual areas within psychology has been reported in over 40 academic journal articles, including *Nature, Science, and Psychological Bulletin.* Wiseman has published six books, most recently *The Luck Factor* (Century, 2003).



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The 'Miracle Poet' Case

Japanese Media Fooled by the Doman Method and Facilitated Communication

SADAHIKO NAKAJIMA

On April 28, 2002, Japan's nationwide public TV channel (Nihon Hoso Kyokai, NHK) broadcast a program titled *Miracle Poet: Messages From an 11-year-old Boy with Brain Damage.* It is an episode of "NHK Special," the most influential TV documentary series in Japan, with an audience of 14 million. In the program, a speechless boy with severe brain damage "developed" his intelligence by means of the Doman method (described below), "read" 2,000 books including philosophy, physics, and astronomy, and "communicated" with people via Facilitated Communication (FC). According to the narrator, the souls of many people are being healed by the boy's poems and essays, which were "written" with the aid of a letter board and his mother. This program, however, stunned a lot of sensible viewers in disbelief, because his mother grabbed his hand, pushed it to the paper letter board, and read the "selected" letters. Most amazingly, the mother continued to move the boy's hand even when he was yawning and after he fell asleep! She seemed to believe her son's literacy was real. (A portion of the program is available online at http://lunavideo.hp.infoseek.co.jp/index_e.html, and the letterboard pointing at www.geocities.co.jp/Technopolis/9822/test.swf.)

The Doman method is based upon incorrect knowledge of the brain and the unverified assumption that people with brain damage can be cured by continuous exercise and stimulation termed "patterning." In the framework of this method, all sorts of different brain damages yield a blockage of brain development, which can be eliminated by the patterning training. The Doman method has been criticized not only by skeptics (e.g., Hines 1988, 1995, 2001; Novella 1996) but also by academic societies (see the policy statement of the American Academy of Pediatrics, www.aap.org/policy/re9752.html, for example) due to lack of empirical and theoretical validation. This method gives parents of disabled children the illusion that their child may one day live without disabilities and requires a lot of emotional and economic resources. Facilitated Communication is also a harmful and unscientific communicative procedure (see, for example, Dillon 1993; Gardner 2001; Green 1994, 1995; Hudson 1995; Jacobson, Mulick, and Schwartz 1995; Mostert 2001; Mulick, Jacobson, and Kobie 1993; Shane 1994; and Spitz 1997) and policy statements against the use of FC have been issued by several academic societies such as the American Academy of Pediatrics and the American Psychological Association. However, the TV program did not mention any of this. Furthermore, it did not provide any comments by specialists such as neurologists, psychologists, physicians, physiotherapists, or special education teachers. (In marked contrast to the situation in North America, FC is unknown to the public in Japan, and only a handful of psychologists are conversant with its controversial history.)

Sensible viewers began to voice their doubts on Web pages immediately after the program, and continuing discussions lasted for several months. More than 2,000 phone calls, fax messages, and e-mails were sent to NHK within a week. Because NHK is funded by public money, it is legally prohibited from promoting specific products from private enterprises. However, the program appears to have links with a major company that published that boy's book, *Rules In Which Nobody is Denied* (in Japanese), offered for sale immediately after the broadcast. Weekly and monthly magazines carried stories against NHK. Newspapers, however, took a wait-and-see attitude and gave generally neutral accounts, though some papers printed comments by informed people who criticized NHK.

Conventionally, the episodes of "NHK Special" are rebroadcast in Japan, and they are also broadcast in some cities of the North America via cable networks. NHK decided not to do so with the program Miracle Poet. Nevertheless, NHK did not accept the criticisms and broadcast a brief explanation on May 11. According to NHK's explanation, its TV crew believes that the boy's ability is genuine, and anonymous specialists are said to support this belief. They seemed not to

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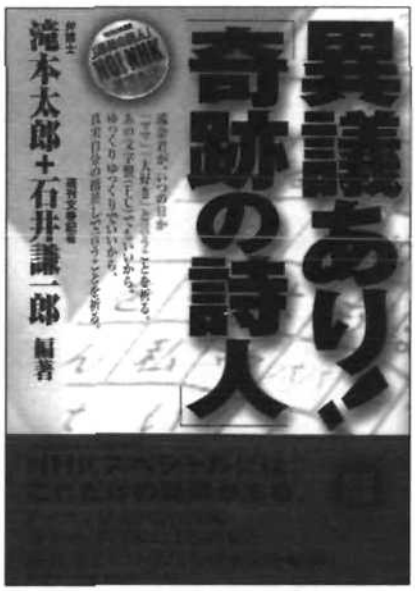
have considered the possibility of the Ouija (or ideomotor) effect. These reactions made sensible viewers so angry that the viewers began campaigns against NHK by creating badges, sticker seals, bookmarks, paper fans, and T-shirts in protest. Furthermore, a large number of Web sites were set up by volunteers against NHK's behavior. Among them is a site where academic and critical papers on the Doman method and FC are translated into Japanese. On June 28, the book *Objection to Miracle Poet* (in Japanese), was published, and about 10,000 copies have been printed through the end of the year. The book was edited by Taro Takimoto, a lawyer working against cult groups such as Aum Shinrikyo, and by Ken-ichiro Ishii, a journalist. The book's main argument against NHK is that the program may have given false hope to a lot of people with disabled family members.

As a board member of the Japan Skeptics and the chair of its psychology committee, I sent a letter of complaint to NHK in the beginning of May and called an urgent conference on June 8 for hearing citizens' voices. Based upon my report on the conference, the Japan Skeptics issued a presidential comment in August, which pointed out two problems with the program (Anzai 2002). First, there is no evidence that the boy's "expression" came from himself. Second, the program violates the Japanese Law of Broadcasting, which says "any program must provide varied views if the topic is controversial." The statement concludes that unscientific behavior such as NHK's may yield a biased view of handicapped people. The Japan Skeptics also carried my reports on the case of Miracle Poet (Nakajima 2002a, 2002b, and 2002c).

There were also movements in academic and professional societies. The Japanese Society of Child Neurology sent a letter of inquiry to NHK at the end of October, and the Japan Pediatric Society, which is the parent organization of JSCN, issued a statement on the official Web site on November 20 that supports the policy statement of the American Academy of Pediatrics concerning the Doman method. The Japanese Speech-Language-Hearing Association published a critical opinion of FC in its journal in February, 2003.

On November 14, a member of the House of Representatives of the Japanese Diet called a managing director of the NHK to a committee of the House, where the NHK was accused of improper programming. In addition, an officer of the Ministry of Health, Labor, and Welfare presented a statement that the Doman method is not proper treatment for brain-damaged people. However, the



Nakajima's book criticizing NHK and the Doman method.

NHK director escaped rebuke by claiming that the program was not intended to disseminate the Doman method. The chairperson of NHK, Katsuji Ebisawa, is still claiming there were no problems in the program. Volunteer groups are still working to spread word of NHK's irresponsible behavior and to disseminate correct information on the Doman method and FC to the general public.

Acknowledgments

I am grateful to many courageous people fighting against NHK on the case of *Miracle Poet*.

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Dowsing Mysterious Sites

I'm not an archaeologist but, as something of a jack of all trades, I have participated in some archaeological investigations and digs, including a forensic one that unearthed hidden skeletal remains and a bullet (Renovation 1981). In short, I know enough to appreciate what a boon psychic power would provide to the field—if such power were actually to exist.

Psychic Archaeology

Certainly, there are many who believe in "psychic archaeology"—the supposed "application of clairvoyance and other psychic skills to the field of archaeology, especially in the location of dig sites and the identification of artifacts." It may involve psychometry (in which an object is used to obtain psychic "impressions"), dowsing (divination with a device such as a rod or pendulum to locate hidden things from a site or map), automatic writing (in which spirits of the dead or other sources supposedly guide the hand to produce messages), or some other alleged psychic mode (Guiley 1991).

What has been termed "perhaps the first, best-known case of deliberate psychic archaeology" was launched in 1907 by Frederick Bligh Bond in excavations at England's Glastonbury Abbey. Unknown to the Church of England officials who appointed him, Bond was an occultist who turned to a friend's automatic writing for help in locating the ruins of two chapels. Soon "Gulielmus Monachus" ("William the Monk") and other spirits, including "watchers from the other side," were tapping the "Universal Memory" to provide the necessary site information. Bond's excavations were successful, but when he eventually revealed his methods in 1917 the Church was embarrassed and he was forced out, ending the work in 1922 (Guiley 1991).

Archaeologist, skeptic, and CSICOP fellow Kenneth L. Feder—in his *Frauds, Myths, and Mysteries* (1996, 198)—points out the problem of possible prior knowledge in such cases. "Bond was an expert on medieval churches," notes Feder, "and we know that he had access to and had examined many of the documents, maps, plans, and drawings of the abbey before initiating field research." Moreover, "Although much of the abbey was a ruin, some walls and foundations were visible at the surface." The locations of the chapels, supposedly identified for Bond by spirits, were already generally identified in old documents, and previous searches had greatly narrowed the areas to be searched. Feder observes that Bond simply "searched for and found the chapels in the only reasonable places left for them to be" (Feder 1996).

Scientific tests have not proved the efficacy of psychic archaeology (Feder 1980; 1995). Such tests "either are not conducted or are conducted so poorly as to render the results meaningless" (Feder 1996).

Some defenders of dowsing would object to its being categorized with psychic claims, suggesting it may have a natural explanation. Dowsers have often attributed the movement of the rods or pendulums to "earth force fields," which supposedly send out "vibrations" such as electromagnetic or other radiations. Yet many dowsers work from maps, far from the actual locations, and some claim to receive images—including those of the past and future—like clairvoyants (Guiley 1991, 155–156). Dowsers have claimed success in locating hidden objects, even missing persons (Nickell 1994).

Unfortunately, dowsers actually fare poorly in properly controlled tests of their abilities, as James Randi (1982; 1991) has repeatedly demonstrated. I once tested some gold dowsers in the Yukon Territory of Canada, site of the famed Klondike gold rush. Alas, they could not differentiate between gold nuggets, fool's gold, chromium nuts and bolts, and empty boxes—although they had rationalizations for their failures (Nickell 1976).

The well-established, scientific explanation for the movement of dowsing rods and pendulums is that it is due to "the unwitting translation of thoughts into muscular action," the same force responsible for various phenomena like automatic writing, table tipping, the movement of a Ouija board's planchette, etc. (Gardner 1957, 109).

Nevertheless, the claims continue.

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Here are two examples—one from Australia, the other from Germany—that involve dowsing to supposedly locate or provide information about alleged archaeological sites.

The Mahogany Ship

Reportedly, a boat carrying three men hunting seals capsized off southeastern Australia in 1836, drowning the captain. The two survivors sought safety along this now aptly named "Shipwreck Coast" (where over eighty shipping disasters occurred between 1836 and 1922). At that site, between present-day Port Fairy and Warrnambool, the two spied a wrecked ship, lodged in the hummocks (or sand dunes) (Lindsay 1996, 2).

Since then, the legend of the Mahogany Ship has challenged historians and intrigued tourists. There is even an eye-catching "Mahogany Ship Replica" (figure 1) at the McDonald's restaurant in Warrnambool—dubbed the "McReplica" by a waggish Bob Nixon, Australian Skeptics' Chief Investigator. Bob, Richard Cadena (Victoria Skeptics vice-president), and I had motored from Melbourne along the Great Ocean Road in search of a mystery.

The enigma is that the wreck was periodically sighted and lost, presumably due to shifting sands, until it finally disappeared, leaving only controversy behind. Worse, it was a perambulating wreck. As Bob Nixon (2001) says:

> It is variously described as close to Port Fairy, mid-way between Port Fairy and Warrnambool, three miles west of Warrnambool. She is said to be high in the hummocks, well above the high tide mark, in the water, between two hummocks, at the end of a gap in the hummocks. She is identified to be in various states of decay, in various orientations, bows pointing west and north. In short there is little agreement among the reports.

The chronology of events is illuminating. Just after the 1836 sighting by the two sealers, an expedition was mounted by a Captain John Mills to recover their capsized boat along with the drowned captain's body. However, the searchers failed to sight the reported shipwreck. Mills again searched in 1843 and did discover a wreck, which he revisited in 1847. There were sightings off and on over subsequent decades, until the wreck was last seen in 1880. It has since become the subject of persistent legend, as in these closing verses of an unattributed poem, "The Ancient Ship" (Lindsay 1996):

Figure 1. Faux "Mahogany Ship" at the McDonald's in Warrnambool, along Australia's "Shipwreck Coast"—dubbed the "McReplica" by Bob Nixon.

> . . . An ancient ship—
> we've heard the tales—
> Was forced ashore by southern gales,
> Nothing left—no masts or sails,
> Just timbers like mahogany.
>
> Will it be discovered soon
> That ancient ship beneath a dune?
> Only the sun, the stars, the moon
> Are witness to this mystery.

Some have thought the legendary wreck was Dutch, Spanish, or Portuguese (Loney 1998, 20), possibly even proof of a prior "secret" discovery of Australia by the Portuguese, rather than by Captain Cook (McIntyre 1977).

There have been skeptics, however. In 1896 a local historian expressed doubt as to the existence of the wreck, stating that most older residents regarded it as mere fable. He cited the use of old timbers, saturated with whale oil, that had been used for flooring in a house; they were mistakenly thought to have been salvaged from the Mahogany Ship but had probably come from "an old whaling punt" (Loney 1998, 17–18).

I was fortunate to be able to investigate the Mahogany Ship mystery with Bob Nixon and Richard Cadena. Bob had been in touch with Peter D'Aloisio, the 1988 "Australian Champion" water diviner (dowser) and professional water driller. D'Aloisio had used his divining rods to locate what he believed was a chain and nine-foot anchor, possibly from the fabled wreck itself. He had "found" the alleged relics in December 1992, and from February to June 1993 (when he was hospitalized with an angina attack) he drilled two holes: One turned up bits of metal; he then sank a large-diameter pipe to a depth of some thirty-three feet, but unfortunately the shaft flooded.

We met Peter, his brother Dominic, and two of their friends in Warrnambool and followed them to the location at Levy's Point Coastal Reserve. Peter told me he had been directed to the general site by local landmarks. He had, he said, spent about $150,000 (Australian) in motel bills and exploration costs. He and a diver had gone down to the bottom of the flooded pipe shaft but could not be certain what was there. Since 1993 the pipe has been sealed with a metal cover (D'Aloisio 2000).

Peter demonstrated his dowsing technique for us at the site. We took photographs (see figure 2) and later discussed the evidence at the Mahogany Ship Restaurant (where I had a delicious kangaroo steak). Peter showed us some papers related to his project, but the report on tests of the metal traces he had obtained by drilling—provided by Monash University—determined they had probably been from one of Peter's own broken drill bits (D'Aloisio 2000; Nixon 2001a).

Not only is the dowsing evidence therefore unsupported, but much other evidence renders unlikely the claim that a Mahogany Ship anchor has been found. For example, while one possible location for the wreck (shown on a widely published map) is appropriately near the beach, the D'Aloisio site is almost a

kilometer inland, a fact difficult to reconcile with a beached ship.

Actually there is evidence that casts doubt on the existence of an ancient Portuguese (or other exploratory caravel's) wreck. As it happens, the "original" 1836 sighting was reported only decades later and there are conflicting, even apparently "doctored" versions of some accounts (McKiggan 1987, 65). Indeed, Hugh Donnelly, who claimed to have been aboard the boat that set out to recover the capsized whale boat and body of the drowned captain in 1836, did not arrive in Australia until five years later (Fawcett n.d.).

Richard, Bob, and I—in addition to visiting the D'Aloisio site, taking photos, and looking over his documents—explored the area in and around Tower Hill. Actually a lake-filled ancient volcano crater, it provided a magnificent view of Armstrong's Bay to the south, the reputed site of the Mahogany Ship. We also did research at the Flagstaff Hill Maritime Museum where we obtained clippings and researched anchors, learning that one from an ancient Portuguese ship would probably have been much smaller than that described by D'Aloisio. We also interviewed a member of the Mahogany Ship Committee, John Lindsay (2000), who told us that extensive negative results of searching with a magnetometer suggest it is unlikely an anchor would be found in the area. Finally, we visited the local library, staying until closing time and photocopying much material. This included information from a book cited in a Mahogany Ship display at the museum—which turned out to be a novel!

Back in Melbourne, Bob Nixon and I visited the state library of Victoria to conduct still further research. We searched out early newspaper accounts on microfilm, notably one from the *Portland Guardian* of October 29, 1847, describing a wreck found near Warrnambool. It was "thrown completely into the hummocks, and buried in the sand," its deck was "completely gone." The account went on to suggest it had been the source of "a number of articles strewed along the beach" in 1841, adding that "from several articles of French manufacture that were then found it was deemed that the vessel had been a French whaler."

Bob and I have come to believe that this French whaler was a plausible—I will even say probable—candidate for the Mahogany Ship. Given the differing descriptions and locations, however, it appears that more than one wreck could be involved. In any case, to date there is no evidence that dowsing has added anything other than confusion to the shipwreck mystery.

Figure 2. Peter D'Aloisio dowsing the site where he believes may lie the anchor of the Mahogany Ship. (In background, left to right, are Bob Nixon, Richard Cadena, Peter's brother, and a friend.)

Celtic Sites

During an investigative tour of Germany in the fall of 2002 (Nickell 2003a; 2003b), which included some locales in Franconia (northern Bavaria), I was able to explore three sites associated with the legendary Druids, the Celtic priesthood.

The expedition, on a bleak, drizzly October 13, was arranged at the request of my excellent German guide, Martin Mahner, Executive Director of the Center for Inquiry–Europe. Accompanying us were several intrepid members of the Bamberg Skeptics Guild and our distinguished leader, geologist Michael Link, from the Paleontological Institute of the University of Erlangen. Michael was conducting the three-site tour in English especially for me, and I am indebted for that and many additional kindnesses from him and others (see Acknowledgments).

From Bamberg, our little convoy proceeded some fifty kilometers southeast into the area known to tourists as "Franconian Switzerland" for its mountainous terrain, caves, castle ruins, and other scenic features. It was once inhabited by the Celts, a tribe that extended into central Europe about 1200 B.C.

Our first stop took us to a remote wooded hill known as Ringwall. Had we not been with Michael Link, we would have thought it only a scenic place with some natural rock outcroppings. Once shown, however, we could see that, in addition, covered with thick moss and a stand of trees, were distinct earthworks. They had been added to the limestone formations to create what was, indeed, an ancient Celtic fortress—hence the name Ringwall ("circular rampart"). Michael informed us that excavations at the site turned up iron implements and pottery dating to ca. 500 B.C.

From Ringwall we proceeded to another isolated site, about 1.5 kilometers further southeast, known as Espershöhle (*höhle* meaning "cavern"). This is a cave of Jurassic limestone from which cold air emanates, giving it the popular name Eishölle (i.e., "Ice Hell"). It may have had mystical meaning for the Celts. Having been an avid spelunker during my college years, I was Michael Link's apt pupil when he pointed out that the amphitheater-like entranceway resulted from a cave "room" having collapsed and over time becoming exposed at the surface (see figure 3). A passageway (which is home to hibernating bats from October until April) leads to a fifteen-meter-deep pit. This was excavated by archaeologists in 1937–1938, and the recovered artifacts identified it as a Celtic site. The presence of skeletal remains—some bones even exhibiting knife marks—seemed to indicate the practice of sacrifice (Link 2002).

The last of the three sites we visited on our expedition is known as Druidenhain

or "Druid's Grove." The arrangement of giant rocks that litter the wooded area, interspersed with passageways for a labyrinthine effect, have inspired popular belief that it is an ancient Celtic site. Some have called it "The University of the Druids." Supposedly, the Celts subjected the rock to heating and cooling to produce fracturing and thus create the "monoliths," a notion leading to another sobriquet for the place, "The Franconian Stonehenge" (Link 2002).

Imaginative names have been given to the formations, such as "Christening Stone," "Bowl Stone," "Entrance to the Underworld," and so on. The belief that the place was one of sacrifice—an idea that dates from 1863—led to one boulder being styled the "Sacrificial Stone."

Dowsers eventually got into the game and in 1983 determined that the "Altar Stone" was at the intersection of two "earth-ray" lines. These are apparently similar to the earth-energy or "ley" lines that are imagined to connect mystical sites; leys represent an idea advanced by English beer salesman Alfred Watkins (1925), an amateur antiquarian. Dowsing is often touted as a method of supposedly detecting such "earth energies" (Guiley 1991, 157).

Dowsing was also used in other ways at Druidenhain. For example, dowsers employed their witching wands to determine the supposedly true nature of one of the supposed megaliths. As a consequence, it was bestowed with the fanciful name, "The Grave" (Link 2002).

Alas for the evidence from dowsing, and for the belief that the site is Celtic or even manmade, Druidenhain has yielded no potsherds, skeletal remains, or other evidence of human habitation. Moreover, the array of "monoliths" is actually a natural formation—the product of geologic forces and erosion. (During the geologic formation of the mountain, pressure caused the rock to fracture, producing numerous faults crisscrossing each other. These faults were then attacked preferentially by seeping water, with the erosion along the lines eventually resulting in a multitude of rocks aligned in rows [Link 2002].)

Conclusions

As both the Mahogany Ship and "Druidic Grove" cases demonstrate, dowsing does not seem an effective method of archaeological investigation—whether considered a "psychic" means or not.

However, not only is science an ongoing process, but so is popular belief. From either perspective, it is important to continue to look at significant new cases and

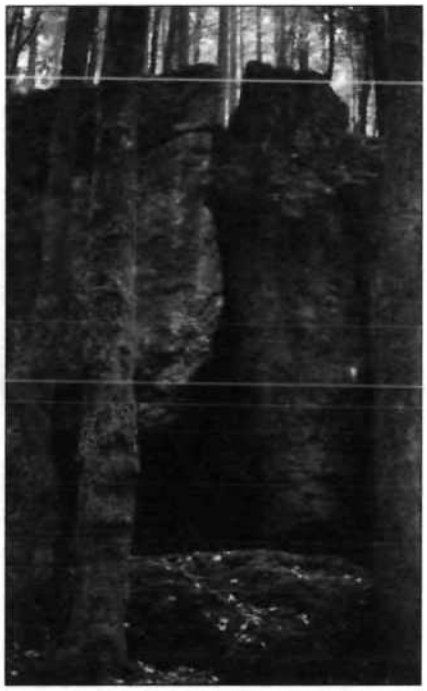

Figure 3. Entrance to German cave with a 15-meter-deep pit that yields evidence of apparent Druidic sacrifice.

to even work the "cold case" files—not with the view of mystery mongers to promote them, or of self-styled "debunkers" to dismiss them, but as investigators trying to understand and explain them. In so doing we can learn more about ourselves and our world—a reward dogmatists often seem unaware of.

Acknowledgments

In addition to those mentioned in the text, I am grateful for the assistance of Barry Williams and Ian Bryce (in Australia) and Amardeo Sarma, Christoph Bördlein, and Detlev Lück (in Germany). Closer to home, Paul Kurtz, Barry Karr, Tim Binga, and Ranjit Sandhu, along with Benjamin Radford, Kevin Christopher, and the entire Center for Inquiry support staff, all deserve thanks for helping in various ways. To John and Mary Frantz, for their creation of an investigations fund that helps make such sojourns possible, I am continually grateful.

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'Elementary, Dear Watson'

It turns out that Sherlock Holmes never says "Elementary, dear Watson!" in any of Sir Arthur Conan Doyle's original stories. Apparently, the phrase was introduced with the first theatrical representations of the fictional detective, with the consent of Doyle in reply to a letter by the actor William Gillette, who originally played Holmes on the stage. More important, as far as skeptics are concerned, the type of reasoning employed by Sherlock Holmes was not "deduction" at all, though Doyle referred to it that way. In fact, in all his adventures Holmes uses what in philosophy of science is referred to as "induction to the best inference." It is important for anyone interested in critical thinking and science to understand the difference between deduction and induction, and the strengths and pitfalls of the two approaches.

The type of reasoning employed by Sherlock Holmes was not "deduction" at all, though Doyle referred to it that way.

Deduction is a form of reasoning with which one can go from general premises to specific predictions. It was first formalized in Western philosophy by Aristotle, who described a simple variety of it known as syllogism. From the two premises "All men are mortal" and "Socrates is a man" one can *deduce* the conclusion that "Socrates is mortal" (a conclusion that, unfortunately, was given final proof when Socrates was condemned to die by the Athenian state for teaching critical thinking to the local youth). Deduction is particularly useful in mathematics and to formulate predictions (hypotheses) based on general scientific theories. Notice, however, that while deduction is truth-conservative, it is not truth-ampliative. What this means is that, *if* the premises are true (and the deductive sequence is formally correct), then the conclusion is guaranteed to be true (i.e., the truth of the premises is conserved). However, a deductive reasoning does not augment our knowledge of the world (it is not ampliative), it simply makes explicit what is already contained in the premises. While the latter is often a valuable enterprise, it clearly is not the sort of thing that would help Holmes solve a crime, or a scientist make generalizations about the world.

For the latter, we need a different kind of reasoning, often referred to as induction. Induction is, in some sense, the opposite of deduction: it seeks to go from particular facts to general statements. However, the price to pay is that induction is not necessarily truth-preserving: we can generalize from specifics, but the generalization may turn out to be wrong. Bertrand Russell, with his characteristically dry humor, nicely explained this: he imagined an "inductivist turkey" which is brought to a farm and fed regularly every morning at the same time. The turkey wishes to make predictions about his future, but—being a good inductivist—realizes that he needs a large sample of data before being able to do so confidently. So he collects data on when he is fed, how often, and with what. After 364 days, the turkey feels confident that he has enough particular examples to draw a general conclusion:

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He will be fed every morning at the same time, with the same amount and type of food. Sadly, the following day was Thanksgiving, and the turkey was instead slaughtered and brought to the farmer's table. Such are the perils of the non truth-preserving character of induction.

And yet induction is a quintessential component of scientific investigations. Indeed, the argument can be made that even deductive reasoning does, ultimately, rely on induction: after all, one has to get the premises from somewhere, and this is usually done by induction. For example, while we can establish that "Socrates is a man" by direct observation, the premise that "All men are mortal" is neither the result of simple observation nor a necessary piece of logical inference. The only reason we think that all men are mortal is because every man we have seen *so far* eventually died. But—for all we know—we may be in the same position as Russell's turkey and drawing unwarranted conclusions from insufficient evidence (notice, incidentally, that even "simple observations" are made possible by the inductive inference that our senses—usually—are reliable).

Skeptic philosopher David Hume was the first one to recognize this "problem of induction," i.e., that all our knowledge, ultimately, is based on inductive generalizations, even though we have no independent validation of induction itself. One might think to get around the problem of induction by taking a pragmatic approach and stating that we use induction because it has worked well so far. Sure, but that in itself is a piece of inductive reasoning. You see the problem.

Famously, Karl Popper thought he had solved the problem of induction by stating that science does not seek to prove its theories, only to disprove them. This is the well-known principle of falsification. I will eventually devote a whole column to it, given its importance in skepticism and science, but for now let us say that most philosophers of science think that simple falsificationism cannot work for the reason that we can never test hypotheses in isolation. Every time we allegedly attempt to falsify a hypothesis, in reality we are also testing several corollaries and assumptions, and at least some of them will have been derived via induction.

Having said this, I wouldn't throw away science or skeptical investigation just because induction cannot be justified from within science. There are solid philosophical reasons to think that science is on firm grounds as a method of inquiry, and there are plenty of *results* that assure us that it works. But the problem of induction is a good reminder that even skeptics and scientists do have to accept certain philosophical assumptions to do their work. If such assumptions make you a bit uncomfortable, this just adds to the fun.

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Special Forces Battle Giant Scorpions in Iraq

According to a report first published December 17, 2002, in Joseph Trainor's UFO Roundup (http://ufoinfo.com/roundup/), a UFO may have crashed in Iraq—leaving Saddam Hussein in possession of extraterrestrial technology that may be turned against invading forces. The San Francisco Bohemian physicist Jack Sarfatti passes on information that America's "greatest fear is that Saddam will reverse-engineer the crashed alien spacecraft. . . . The craft allegedly crashed during the Gulf War (1990–1991) or more recently (possibly December 1998). This will be Iraq's Roswell. The U.S. is currently reverse-engineering the Roswell craft and fears Saddam's scientists will catch up with or even go beyond the U.S. in one or more areas. These areas of research include zero point, over-ratio or gravimetric technology, which would allow for a tremendous advance, allowing Iraq to become a leading power."

Soldiers prepare for battle in Iraq.

The Arab reporter Mohammed Hajj al-Amdar writes, "Saddam gave the aliens sanctuary, so that they couldn't be captured by Americans. Nobody can reach the citadel Qalaat-e-Julundi at night. They say that the aliens created 'watchdogs' for Saddam. The aliens took ordinary desert scorpions and used their bioengineering to grow the scorpions to giant size. Scorpions of a cow-size! They are wonderful watchdogs: they blend in with the desert, swiftly and silently move on their warm-blooded prey for a decisive attack. Luckless intruders hear just some strange sound from behind stones, then a pincer crushes their necks, another pincer crushes their legs; then the victim is slammed to the ground and beaten with a barbed tail six or seven times. Death comes almost immediately. . . . I hear that Saddam says the mission of these creatures is 'to discourage guests who have not been invited.'"

The story about Saddam's crashed UFO was picked up by the Russian newspaper *Pravda* on January 31, citing Trainor. *Pravda* adds to the story by citing dubious sources who claimed that "A high-ranking source admitted that U.S. Air Force's F-16 brought down a UFO over Saudi Arabia during the Operation Desert Storm, and five countries are trying to conceal information about this fact. . . . It was a relatively small craft, of approximately fifteen feet in diameter. It had three chairs, probably for crew members, but they were so small as if meant for children. To all appearance, space aliens were just about three feet tall. However, it seems incredible that there were no dead bodies at the crash site." *Pravda* leaves itself an "out" by suggesting, "If no additional information follows in connection with the case, it may be still considered just doubtful anonymous rumors," leaving one to wonder why the newspaper published them in the first place.

Robert Sheaffer's World Wide Web page for UFOs and other skeptical subjects is at www.debunker.com.

The best-selling horror and fantasy writer Whitley Strieber, who also writes supposedly true accounts about his encounters with alien-like entities, placed the story of Saddam's Scorpions on his Web site (www.unknowncountry.com/news/?id=2351). Strieber suggests that Saddam may have planted the crashed saucer stories to scare people away from Qalaat-e-Julundi, where his henchmen may be making banned weapons.

* * *

If the messages received from the Zetas by Nancy Lieder are correct, by the time this is published, the whole world will be staring up at the huge red object in the sky. And we don't mean the planet Mars, which this summer will be drawing unusually close to Earth and has undoubtedly already startled many early-risers. We refer instead to a supposed Planet X, which according to the Zetas is now approaching Earth to cause a "pole shift" and massive destruction on or about May 15 (see this column, November/December 2002). "The Twelfth Planet will be visible to the common man some seven weeks prior to the shift, without the use of telescopes," says Nancy, meaning that it should have become visible around the end of March. "Planet X will have a distinct red appearance, with a roiling tail full of moons, that are more concerned with the dance between them than any effect the solar wind might have upon them. Thus, they swirl, and look like a dragon approaching, not a straight line tail at all. Nevertheless, we anticipate NASA will explain the Planet X complex as any number of things, or rather their lackeys, who will natter the word on every Internet or media source that allows their nattering—asteroid bunch, passing comet [C2002/X5], unusual comet [C2002/V1], Mars closest pass in many eons, or whatever." In other words, by May "Planet X" will be huge and impossible to miss, but the government will attempt to convince you that you are actually seeing something else.

At the time of this writing, Nancy still appears supremely confident that her prophecy of doom will indeed take place. She notes on her Web site (which claims to be in the top five percent of sites, and it wouldn't surprise me if it is) that last December, the popular late-night radio host Art Bell interviewed Sylvia Browne, "the well-known psychic and author. And she disclosed she is no longer able to see the future beyond the summer of 2003." Bell also interviewed Ed Dames, who claims to have been a "remote viewer" employed by the CIA. Dames said that "he and his remote viewing team are no longer able to remote view the future beyond next summer." From this, Nancy deduces that there will *be* no future beyond this summer.

If the messages received from the Zetas by Nancy Lieder are correct, by May "Planet X" will be huge and impossible to miss, but the government will attempt to convince you that you are actually seeing something else.

It will be instructive to check Nancy's Web site, www.zetatalk.com, after her Doomsday has come and gone to see what explanation she offers for the fact that the Earth is still intact. Some possibilities: (1) She misinterpreted the Zetas' messages, and the destruction is coming at a later date; or (2) the cosmic near-collision has already occurred, and we didn't notice it; or (3) she may say nothing, do nothing, and just slink away.

But just because Earth has narrowly avoided destruction at the hands of a planet that failed to show up for doom as scheduled does not mean that doomsday won't soon be upon us. In another issue of Trainor's *UFO Roundup* (October 15, 2002), he notes that "Neo-Nazis, Black Sun enthusiasts and a handful of esoteric types believe that Hitler's Germany made contact sixty years ago with an alien civilization on a planet orbiting the star Aldebaran, sixty-eight light-years from Earth." (According to some contemporary neo-Nazi conspiracy theories, the Aryans actually arrived thousands of years ago in spaceships from Aldebaran.) "In a last-ditch effort to stave off defeat, the Nazis launched a crude spaceship, the Vril-Odin, which traveled to Aldebaran, seeking military assistance against the Allies. The ship supposedly arrived in 1967." If this is true, then the world's first manned space voyage was interstellar, and it traveled faster than light. At this very moment, an "Aldebarani space armada" consisting of over 200 heavily armed battle cruisers and the like is supposedly on its way to Earth to deal World War II's victorious allies a crushing defeat. But since this information comes from a book published by a notorious holocaust denier, I wouldn't bet money on its veracity.

Trainor has more to say about "saucer Nazis" in the item about Saddam's Scorpions. He notes that "Nazi interest in Iraq goes back eighty years to the early 1920s." Supposedly the early Nazi leader Detlef Schmude went to Iraq in 1924, "and spent the next few years hunting for what he called 'the Hidden Masters.'" In 1940, SS-*Brigadefuhrer* Karl Maria Wiligut "called Himmler's attention to important archaeological discoveries in Iraq's Irbil province. Scientists had dug up man-made artifacts dating back to 10,000 B.C. Wiligut claimed to be the last of a line of sorcerers called the Ueiskuinigs, which had originated hundreds of thousands of years ago on the lost continent of Atlantis." I am not clear where all of this is supposed to lead, except perhaps to suggest that the Indiana Jones stories are actually nonfiction. Trainor concludes, "It appears that something is going on in the Little Zab valley, but it is unclear just what. Is Zarzi the site of Saddam's 'Area 51?' Or the location of some Neolithic 'Temple of Doom?'" □

I Remember Doing the Time Warp

The Incredible Story of Father Ernetti's Chronovisor

Marco Tullio Cicero, the great lawyer, orator, politician, and philosopher, gave a speech to the Roman senate in 63 B.C. and an observer noted: "His gestures, his intonation; how powerful they were! What flights of oratory!"

These comments, however, are not from a contemporary of Cicero but instead from Father Pellegrino Ernetti, an Italian Benedictine monk born in 1925 and who died in 1992. Logically, similar appreciations on gestures and intonations could only be possible by observing the said orator in action; this is exactly what Father Ernetti claimed he had done. Not only that, the monk was also apparently able to witness one of Napoleon's speeches, a Latin tragedy of 169 B.C., and even the passion of Christ on the cross.

Just another mystic visionary? Not at all. Father Ernetti was a musician, a celebrated historian of archaic music at Venice's Conservatorio di Stato Benedetto Marcello, a philosopher, and a physics studies graduate. It was thanks to his scientific interest that, in the 1950s, he was able to build the "Chronovisor," a true time machine!

Unlike the fantastic one imagined by H.G. Wells, however, the Chronovisor could not transport people back and forth in time but was said to allow the user to see historical events in the exact moment they took place, like a sort of tridimensional television.

Massimo Polidoro is an investigator of the paranormal, author, lecturer, and co-founder and head of CICAP, the Italian skeptics group.

As Time Goes By

Father Ernetti, who was also an exorcist of considerable renown, claimed that the Chronovisor was the result of many years of study by a team of scientists that, apart from himself, included twelve famous people who, he said, preferred to be anonymous. The only names he let slip were those of physicist and Nobel Laureate Enrico Fermi and of rocket scientist Wernher von Braun.

"First of all we wanted to verify that what we saw was authentic," Ernetti told Father François Brune, a French theologian and author who had befriended the Italian monk. "So we started off with a relatively recent scene of which we had much documentation and footage. We tuned the machine on one of Mussolini's speeches. Then we started to go backward and observed Napoleon giving the speech in which he proclaimed Italy a republic. We then traveled much further back in time, to ancient Rome. First, there was a bustling fruit and vegetable market in the time of Emperor Trajan. Next, a speech by Cicero, one of the most famous, the first delivered against Catilina." Ernetti said that he had noticed slight differences in the Latin pronunciation of Cicero's time as compared to the Latin taught in schools today.

Next, the time-travelers "dallied," as Ernetti put it, "at a playlet." The year was 169 B.C.; they watched part of a tragedy, *Thyestes*, written by the "father of Latin poetry," Quintus Ennius. It was a play, explained Ernetti, that is now almost wholly lost to us; only twenty-five fragments, a line or so each, have survived.

"Have you been able to reconstruct what you heard?" asked Father Brune. "Yes," replied Father Ernetti enthusiastically. "Since we heard and saw everything, text, choruses, music, I've been able to publish the entire text of the tragedy."

Ernetti appeared to be very reticent to give details about the machine's invention. "It happened virtually by accident. . . . The basic idea was very simple. It was just a matter of stumbling upon it."

And who exactly invented it? "No one person" replied Ernetti. It had been a joint creation, where Fermi played a seminal role.

Father Ernetti said the Chronovisor consisted of three components. First, a multitude of antennae, which were able to pick up every conceivable wavelength of light and sound. These antennae were made of alloys consisting of three mysterious metals. The second component was a type of direction finder, activated and driven by the wavelengths of light and sound which it received. You could set it to a given place, date, and even person of your choice. The third component was an extremely complex array of recording

devices, which made possible the recording of sound—and particularly of images—from any time and any place (Brune and Chauvin 1993).

Jesus Christ Superstar!

Aside from these engaging tales, however, the only solid fact was that nobody had ever seen the Chronovisor; the only proof of its existence was Father Ernetti's words.

On May 2, 1972, the weekly Italian magazine *La Domenica del Corriere* (Courier's Sunday) published a picture which Father Ernetti claimed was obtained through the Chronovisor: the image showed Christ's face in agony on the cross.

"At first," explained Father Ernetti to Brune, "we tried to recapture the images of the day of Christ's crucifixion. But we had a problem. Crucifixions, as awful as they were, were commonplace in Christ's time. People were nailed to the cross every day. It also didn't help that Christ wore a crown of thorns, because, contrary to popular belief, it wasn't unusual to be punished by having a crown of thorns put on your head."

They were thus obliged to go a few days further back in time, to the last supper of Christ. "We saw everything" said Father Ernetti simply. "The agony in the garden, the betrayal of Judas, the trial—Calvary." The Chronovisor team brought back a record of this experience: "We filmed it—losing the fine details, of course, but filming it was the only way to preserve it."

No trace of this film, however, ever came to light. The only objective proof that came out of this story was that picture. A few months after its publication, however, the mystery was solved: in the August 1972 issue of the *Giornale dei Misteri* (Journal of Mysteries) a letter and a photo were published. A reader, Alfonso De Silva, explained that he had purchased the photo for 100 lire in the gift shop of the Santuario dell'Amore Misericordioso (Sanctuary of Merciful Love) in the town of Collevalenza, near Todi and Perugia. It was a photograph of the face of Christ on a woodcarving adorning the sanctuary. The woodcarving was by a Spanish sculptor named Cullot Valera.

This photo and the one produced by Father Ernetti were identical, except that one was the mirror image of the other. No one could deny that they were identical, not even Father Brune who, when the monk had less than a year to live, asked his friend about this compromising photo.

Supposed photograph of Jesus by Father Pellegrino Ernetti.

Ernetti's reply was very disingenuous: "He explained that he was aware of the other photo, aware that it was the work of a Spanish sculptor. He also said he knew that the Spanish sculptor had carved his Christ according to the instructions of a certain Spanish nun, that this Spanish nun had been a mystic who carried the stigmata of Christ on her body and was consumed by ecstatic visions of Christ's Passion."

Ernetti seemed to have assumed that Father Brune would understand the rest. The Parisian priest does not dwell on this in his article but Peter Krassa, author of *Father Ernetti's Chronovisor*, does and assumes that Father Ernetti is tendering the following explanation: The ecstatic vision of Christ's Passion which the mystical nun had enjoyed was a vision of Christ dying on the cross on Golgotha. This was the vision she had communicated to the Spanish sculptor; following her instructions, he had sculpted on the face of the Christ on the wooden cross the exact features, the very expression of Christ, that she had seen in her vision! The fact that not only were the pictures identical in their subject, but also in the shadows and light reflections, means that the two images were in fact the same picture (Krassa 2000).

Sunset Boulevard

No trace of the Chronovisor exists and Father Ernetti was very careful to explain that, after its experimental runs, it had been disassembled and "hidden in a safe place."

But why hide such a discovery, Father Brune asked. Father Ernetti's replied, "This machine can tune in on everyone's past completely, leaving nothing out. With it, there can be no more secrets; no more state secrets, no more industrial secrets—no more private lives. The door would be wide open for the most fearsome dictatorship the world has ever seen. We ended up agreeing to dismantle our machine" (Brune and Chauvin 1993).

What to make, then, of such an incredible tale? "As a matter of fact," says Peter Krassa in a recent interview in *Fortean Times*, "there is no reliable witness to Ernetti's claims. No one ever saw the Chronovisor, not even Brune or Senkowski, who were both in close contact with him. Ernetti never named the scientists who were cooperating with him because he wanted, he claimed, to protect them from public harassment. The exceptions were Wernher von Braun and Enrico Fermi, who were already dead" (Heinzerling 2002).

Aside from the photograph of Christ, which turned out to be a fake, the only other "proof" of the existence of the Chronovisor was the transcript of the Thyestes, the lost tragedy. Actually, this one as well, after close scrutiny by Katherine Owen Eldred, who holds a Ph.D. in Classics from Princeton University, provides reasons to doubt. For

one thing, a number of words in the text do not appear in the Latin language until at least 250 years later. Also, says Eldred, there are certain words which are reused in this text too often, clear sign of a limited Latin vocabulary—which was certainly not the case with author Quinto Ennio.

Furthermore, of the twenty-four fragments of Ennio's Thyestes which have been preserved for us by later commentators such as Cicero, Nonius, and Statius, more than half show up in the Ernetti Thyestes piece. Since the Ernetti Thyestes playlet is only one-tenth the probable length of the complete tragedy by Ennius, it might have been expected that, on the average, about ten percent of the fragments would show up. Here 65 percent of the fragments show up—about seven times as many as you might reasonably expect (Krassa 2000).

Near the end of the Chronovisor saga, as in a classic mystery tale, comes an unexpected surprise: a confession. In his book, Krassa reproduced a letter from an unidentified "distant relative" of Father Ernetti who claimed he had met him on his deathbed. Ernetti wished to leave this world with a clear conscience, and so he admitted that "he had not really brought that play back on the Chronovisor. He said that he thought he had composed the play himself, using many fragments that were preserved in the writings of other authors—but he could only very obscurely remember doing that." However, before taking this as a true confession one must know that a few sentences later Father Ernetti tells about another lifetime in which he was a contemporary of Nostradamus. "I knew him. He was an alchemist and a physicist. He, too, experimented with a Chronovisor. It was he who taught me that it might be possible."

Was Ernetti the culprit of a pious fraud, wishing that his tales could lend credibility to Christianity? Or was he the victim of a monster that he created and could not kill?

Was Ernetti the culprit of a pious fraud, wishing that his tales could lend credibility to Christianity? Or was he the victim of a monster that he created and could not kill, like the Fox sisters who started out by playing a joke on their parents and ended up inventing Spiritualism? We will never know . . . unless, of course, someday, someone will come forward with a time machine and will let us go back in time and ask Ernetti for the truth.

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Skeptic Pitied

FAYETTEVILLE, AR—Craig Schaffner, 46, a Fayetteville-area computer consultant, has earned the pity of friends and acquaintances for his tragic reluctance to embrace the unverifiable, sources reported Monday.

"I honestly feel sorry for the guy," said neighbor Michael Eddy, 54, a born-again Christian. "To live in this world not believing in a higher power, doubting that Christ died for our sins—that's such a sad, cynical way to live. I don't know how he gets through his day."

Coworker Donald Cobb, who spends roughly 20 percent of his annual income on telephone psychics and tarot-card readings, similarly extended his compassion for Schaffner.

"Craig is a really great guy," Cobb said. "It's just too bad he's chosen to cut himself off from the world of the paranormal, restricting himself to the limited universe of what can be seen and heard through empirical evidence."

Also feeling pity for Schaffner is his former girlfriend Aimee Brand, a holistic and homeopathic healer who earns a living selling tonics and medicines diluted to one molecule per gallon in the belief that the water "remembers" the curative properties of the medication.

"Don't get me wrong—logic and reason have their place," Brand said. "But Craig fails to recognize the danger of going too far with medical common sense to the exclusion of alternative New Age remedies like chakra cleansing and energy-field realignment."

Eddy said he has tried repeatedly to pull Schaffner back from the precipice of lucidity.

"I admit, science might be great for curing diseases, exploring space, cataloguing the natural phenomena of our world, saving endangered species, extending the human lifespan, and enriching the quality of that life," Eddy said. "But at the end of the day, science has nothing to tell us about the human soul, and that's a critical thing Craig is missing. I would hate for his soul to be lost forever because of a stubborn doubt over the actual existence and nature of that soul."

Gina Hitchens, a lifelong astrology devotee, blamed Schaffner's lack of faith on an accident of birth.

"Craig can't entirely help himself, being a Gemini," Hitchens said. "Geminis are always very skeptical and destined to feel pain throughout life as a result of their closed-mindedness. If you try to introduce Craig to anything even remotely made-up, he starts going off about 'evidence this' and 'proof that.' If only the poor man were open-minded enough to stop attacking everything with his brain and just once look into his heart, he'd find all the proof he needed. But, sadly, he's unable to let even a little bit of imagination drive his core beliefs."

Craig Schaffner, skeptic.

Perhaps the person who pities Schaffner most is his brother Frank, a practicing Scientologist since 1991.

"It's bad enough when someone has the ignorance to reject Dianetics in spite of its tremendous popularity," Frank said. "But Craig isn't even willing to try a free introductory course. Scientology has the potential to free humanity from the crippling yoke of common sense, unshackling billions from the chains of century after century of scientific precedent, and yet he still won't give it a try."

"I realize that Craig seems very happy with his narrow little common-sense-based worldview," Frank continued, "but when you think of all the widely embraced beliefs that are excluded by that way of thinking, you have to feel kind of sad." □

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The Luck Factor

A ten-year scientific study into the nature of luck has revealed that, to a large extent, people make their own good and bad fortune. The results also show that it is possible to enhance the amount of luck that people encounter in their lives.

RICHARD WISEMAN

Barnett Helzberg Jr. is a lucky man. By 1994 he had built up a chain of highly successful jewelry stores with an annual revenue of around $300 million. One day he was walking past the Plaza Hotel in New York when he heard a woman call out, "Mr. Buffett" to the man next to him. Helzberg wondered whether the man might be Warren Buffett—one of the most successful investors in America. Helzberg had never met Buffett, but had read about the financial criteria that Buffett used when buying a company. Helzberg had recently turned sixty, was thinking of selling his company, and realized that his might be the type of company that would interest Buffett. Helzberg seized the opportunity, walked over to the stranger and introduced himself.

The man did indeed turn out to be Warren Buffett, and the chance meeting proved highly fortuitous because about a year later Buffett agreed to buy Helzberg's chain of stores. And all because Helzberg just happened to be walking by as a woman called out Buffett's name on a street corner in New York.

Helzberg's story illustrates the effect of luck in business, but good fortune also plays a vital role in all aspects of our lives. Stanford psychologist Alfred Bandura has discussed the impact of chance encounters and luck on people's personal lives. Bandura noted both the importance and prevalence of such encounters, writing that "some of the most important determinants of life paths often arise through the most trivial of circumstances." He supports his case with several telling examples, one of which was drawn from his life. As a graduate student, Bandura became bored with a reading assignment and so decided to visit the local golf links with a friend. Just by chance, Bandura and his friend found themselves playing behind two attractive female golfers, and soon joined them as a foursome. After the game, Bandura arranged to meet up with one of the women again, and eventually ended up marrying her. A chance meeting on a golf course altered his entire life.

In short, lucky events exert a dramatic influence over our lives. Luck has the power to transform the improbable into the possible, to make the difference between life and death, reward and ruin, happiness and despair.

The Power of Superstition

People have searched for an effective way of improving the good fortune in their lives for many centuries. Lucky charms, amulets, and talismans have been found in virtually all civilizations throughout recorded history. Touching ("knocking on") wood dates back to pagan rituals that were designed to elicit the help of benign and powerful tree gods. The number thirteen is seen as unlucky because there were thirteen people at Christ's last supper. When a ladder is propped up against a wall it forms a natural triangle which used to be seen as symbolic of the Holy Trinity. To walk under the ladder would break the Trinity and therefore bring ill fortune.

Many of these beliefs and behaviors are still with us. In 1996, the Gallup Organization asked 1,000 Americans whether they were superstitious. Fifty three percent of people said that they were at least a little superstitious, and 25 percent admitted to being somewhat or very superstitious. Another survey revealed that 72 percent of the public said that they possessed at least one good luck charm. Superstitious beliefs

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Rob Pudim

and behaviors have been passed down from generation to generation. Our parents told us about them and we will pass them on to our children. But why do they persist? I believe that the answer lies in the power of luck. Throughout history, people have recognized that good and bad luck can transform lives. A few seconds of ill fortune can lay waste years of striving, and moments of good luck can save an enormous amount of hard work. Superstition represents people's attempts to control and enhance this most elusive of factors. And the enduring nature of these superstitious beliefs and behaviors reflects the extent of people's desire to find ways of increasing their good luck. In short, superstitions were created, and have survived, because they promise that most elusive of holy grails—a way of enhancing good fortune.

Superstition comes from a time when people thought that luck was a strange force that could only be controlled by magical rituals and bizarre behaviors.

Testing Superstition

There is just one problem. Superstition doesn't work. Several researchers have also tested the validity of these age-old beliefs and found them wanting. My favorite experiment into the topic was a rather strange study conducted by high school student (and member of the New York Skeptics) Mark Levin. In some countries, a black cat crossing your path is seen as lucky, in other countries it is seen as unlucky. Levin wanted to discover whether people's luck really changed when a black cat crossed their path. To find out, he asked two people to try their luck at a simple coin tossing game. Next, a black cat was encouraged to walk across their path, and the participants then played the coin tossing game a second time. As a "control" condition, Levin also repeated the experiment using a white, rather than a black, cat. After much coin tossing and cat crossing, Levin concluded that neither the black or white cat had any effect on participants' luck. Also, skeptics have regularly staged events in which they have broken well-known superstitions, such as walking under ladders and smashing mirrors—all have survived the ordeals intact.

A few years ago I decided to put the power of lucky charms to the test by empirically evaluating the actual effect that they have on people's luck, lives, and happiness. I asked a group of volunteers to complete various standardized questionnaires measuring their levels of life satisfaction, happiness, and luck. Next, they were asked to carry a lucky charm with them and to monitor the effect that it had on their lives. The charms had been purchased from a New Age center and promised to enhance good fortune, wealth, and happiness. After a few weeks everyone in the group was asked to indicate the effect that the charms had had on their lives. Overall, there was absolutely no effect in terms of how satisfied they were with their lives, how happy they were, or how lucky they felt. Interestingly, a few participants thought that they had been especially unlucky, and seemed somewhat relieved that they could now return the charms.

The Luck Project

Superstition doesn't work because it is based on outdated and incorrect thinking. It comes from a time when people thought that luck was a strange force that could only be controlled by magical rituals and bizarre behaviors.

Ten years ago I decided to take a more scientific investigation into the concept of luck. I decided that the best method was to examine why some people are consistently lucky whilst others encounter little but ill fortune. In short, why some people seem to live charmed lives full of lucky breaks and chance encounters, while others experience one disaster after another.

I placed advertisements in national newspapers and magazines, asking for people who considered themselves exceptionally lucky or unlucky to contact me. Over the years, 400 extraordinary men and women have volunteered to participate in my research; the youngest eighteen, a student, the oldest eighty-four, a retired accountant. They were drawn from all walks of life—businessmen, factory workers, teachers, housewives, doctors, secretaries, and salespeople. All were kind enough to let me put their lives and minds under the microscope.

Jessica, a forty-two-year-old forensic scientist, is typical of the lucky people in the group. She is currently in a long-term relationship with a man who she met completely by chance at a dinner party. In fact, good fortune has helped her achieve many of her lifelong ambitions. As she once explained to me, "I have my dream job, two wonderful children, and a great guy that I love very much. It's amazing, when I look back at my life I realize that I have been lucky in just about every area." In contrast, the unlucky participants have not been so fortunate. Patricia, twenty-seven, has experienced bad luck throughout much of her life. A few years ago, she started to work as cabin crew for an airline, and quickly gained a reputation as being accident-prone and a bad omen. One of her first flights had to make an unplanned stop-over because some passengers had become drunk and abusive. Another of Patricia's flights was struck by lightning, and just weeks later a third flight was forced to make an emergency landing. Patricia was also convinced that her ill fortune could be transferred to others and

so never wished people good luck, because this had caused them to fail important interviews and exams. She is also unlucky in love and has staggered from one broken relationship to the next. Patricia never seems to get any lucky breaks and always seems to be in the wrong place at the wrong time.

Over the years I have interviewed these volunteers, asked them to complete diaries, personality questionnaires, and intelligence tests, and invited them to my laboratory to participate in experiments. The findings have revealed that luck is not a magical ability or the result of random chance. Nor are people born lucky or unlucky. Instead, although lucky and unlucky people have almost no insight into the real causes of their good and bad luck, their thoughts and behavior are responsible for much of their fortune.

My research revealed that lucky people generate their own good fortune via four basic principles. They are skilled at creating and noticing chance opportunities, make lucky decisions by listening to their intuition, create self-fulfilling prophesies via positive expectations, and adopt a resilient attitude that transforms bad luck into good.

Lucky people generate their own good fortune via four basic principles. They are skilled at creating and noticing chance opportunities, make lucky decisions by listening to their intuition, create self-fulfilling prophesies via positive expectations, and adopt a resilient attitude that transforms bad luck into good.

Chance Opportunities

Take the case of chance opportunities. Lucky people consistently encounter such opportunities whereas unlucky people do not. I carried out a very simple experiment to discover whether this was due to differences in their ability to spot such opportunities. I gave both lucky and unlucky people a newspaper, and asked them to look through it and tell me how many photographs were inside. On average, the unlucky people took about two minutes to count the photographs whereas the lucky people took just seconds. Why? Because the second page of the newspaper contained the message "Stop counting—There are 43 photographs in this newspaper." This message took up half of the page and was written in type that was over two inches high. It was staring everyone straight in the face, but the unlucky people tended to miss it and the lucky people tended to spot it. Just for fun, I placed a second large message half way through the newspaper. This one announced: "Stop counting, tell the experimenter you have seen this and win $250." Again, the unlucky people missed the opportunity because they were still too busy looking for photographs.

Personality tests revealed that unlucky people are generally much more tense and anxious than lucky people, and research has shown that anxiety disrupts people's ability to notice the unexpected. In one experiment, people were asked to watch a moving dot in the center of a computer screen. Without warning, large dots would occasionally be flashed at the edges of the screen. Nearly all participants noticed these large dots. The experiment was then repeated with a second group of people, who were offered a large financial reward for accurately watching the center dot. This time, people were far more anxious about the whole situation. They became very focused on the center dot and over a third of them missed the large dots when they appeared on the screen. The harder they looked, the less they saw. And so it is with luck—unlucky people miss chance opportunities because they are too focused on looking for something else. They go to parties intent on finding their perfect partner and so miss opportunities to make good friends. They look through newspapers determined to find certain type of job advertisements and as a result miss other types of jobs. Lucky people are more relaxed and open, and therefore see what is there rather than just what they are looking for.

But this is only part of the story when it comes to chance opportunities. Many of my lucky participants went to considerable lengths to introduce variety and change into their lives. Before making an important decision, one lucky participant would constantly alter his route to work. Another person described a special technique that he had developed to force him to meet different types of people. He had noticed that whenever he went to a party, he tended to talk to the same type of people. To help disrupt this routine, and make life more fun, he thinks of a color before he arrives at the party and then chooses to only speak to people wearing that color of clothing at the party! At some parties he only spoke to women in red, at another he chatted exclusively to men in black.

Although it may seem strange, under certain circumstances, this type of behavior will actually increase the amount of chance opportunities in people's lives. Imagine living in the center of a large apple orchard. Each day you have to venture into the orchard and collect a large basket of apples. The first few times it won't matter where you decide to visit. All parts of the orchard will have apples and so you will be able to find them wherever you go. But as time goes on it will become more and more difficult to find apples in the places that you have visited before. And the more you return to the same locations, the harder it will be to find apples there. But if you decide to always go to parts of the orchard that you have never visited before, or even randomly decide where to go, your chances of finding apples will be dramatically increased. And it is exactly the same with luck. It is easy for people to exhaust the opportunities in their life. Keep on talking to the same

people in the same way. Keep taking the same route to and from work. Keep going to the same places on vacation. But new or even random experiences introduce the potential for new opportunities.

Dealing with Bad Luck

But a lucky life is not just about creating and noticing chance opportunities. Another important principle revolved around the way in which lucky and unlucky people dealt with the ill fortune in their lives. Imagine being chosen to represent your country in the Olympic Games. You compete in the games, do very well, and win a bronze medal. How happy do you think that you would feel? Most of us would, I suspect, be overjoyed and proud of our achievement. Now imagine turning the clock back and competing at the same Olympic Games a second time. This time you do even better and win a silver medal. How happy do you think you would feel now? Most of us think that we would feel happier after winning the silver medal than the bronze. This is not surprising. After all, the medals are a reflection of our performance, and the silver medal indicates a better performance than a bronze medal.

But research suggests that athletes who win bronze models are actually happier than those who win silver medals. And the reason for this has to do with the way in which the athletes think about their performance. The silver medallists focus on the notion that if they had performed slightly better, then they would have perhaps won a gold medal. In contrast, the bronze medallists focus on the thought that if they had performed slightly worse, then they wouldn't have won anything at all. Psychologists refer to our ability to imagine what might have happened, rather than what actually did happen, as "counter-factual."

I wondered whether lucky people might be using counterfactual thinking to soften the emotional impact of the ill fortune that they experienced in their lives. To find out, I decided to present lucky and unlucky people with some unlucky scenarios and see how they reacted. I asked lucky and unlucky people to imagine that they were waiting to be served in a bank. Suddenly, an armed robber enters the bank, fires a shot, and the bullet hits them in the arm. Would this event be lucky or unlucky? Unlucky people tended to say that this would be enormously unlucky and it would be just their bad luck to be in the bank during the robbery. In contrast, lucky people viewed the scenario as being far luckier, and often spontaneously commented on how the situation could have been far worse. As one lucky participant commented, "It's lucky because you could have been shot in the head—also, you could sell your story to the newspapers and make some money."

The differences between the lucky and unlucky people were striking. Lucky people tend to imagine spontaneously how the bad luck they encounter could have been worse and, in doing so, they feel much better about themselves and their lives. This, in turn, helps keep their expectations about the future high, and, increases the likelihood of them continuing to live a lucky life.

Luck School

I wondered whether the principles uncovered during my work could be used to increase the amount of good luck that people encounter in their lives. To find out, I created "luck school"—a series of experiments examining whether people's luck can be enhanced by getting them to think and behave like a lucky person.

The project comprised two main parts. In the first part I met up with participants on a one-to-one basis, and asked them to complete standard questionnaires measuring their luck and how satisfied they were with six major areas of their life. I then described the four main principles of luck, explained how lucky people used these to create good fortune in their lives, and described simple techniques designed to help them think and behave like a lucky person. For example, as I noted earlier, without realizing it, lucky people tend to use various techniques to create chance opportunities, and soften the emotional impact of any ill fortune they encounter. During luck school participants were shown how to be more open to the opportunities that surround them, how to break daily routines, and also how to deal more effectively with bad luck by imagining how things could have been worse. I asked my volunteers to spend a month carrying out exercises and then return and describe what had happened.

The results were dramatic. Eighty percent of people were now happier, more satisfied with their lives, and, perhaps most important of all, luckier. Unlucky people had become lucky, and lucky people had become even luckier. At the start of the article I described the unlucky life of Patricia. She was one of the first people to take part in Luck School. After a few weeks carrying out some simple exercises, her bad luck had completely vanished. At the end of the course, Patricia cheerfully explained that she felt like a completely different person. She was no longer accident-prone and was much happier with her life. For once, everything was working out her way. Other volunteers had found romantic partners through chance encounters and job promotions simply through lucky breaks.

Positive Skepticism

After ten years of scientific research my work has revealed a radically new way of looking at luck and the vital role that it plays in our lives. It demonstrates that much of the good and bad fortune we encounter is a result of our thoughts and behavior. More important, it represents the potential for change, and has produced that most elusive of holy grails—an effective way of increasing the luck people experience in their daily lives.

The project has also demonstrated how skepticism can play a positive role in people's lives. The research is not simply about debunking superstitious thinking and behavior. Instead, it is about encouraging people to move away from a magical way of thinking and toward a more rational view of luck. Perhaps most important of all, it is about using science and skepticism to increase the level of luck, happiness, and success in people's lives. □

More Hazards
Hypnosis, Airplanes, and Strongly Held Beliefs

After a single-case history was reported in the psychological literature, I made an unsuccessful attempt to obtain any documents of the case. However, the adventure provided lessons about why some therapists hold so firmly to certain psychological theories and disdain the critical research.

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> *Imagine that a Viennese prankster, to amuse his friends, invented the whole business of the id and Oedipus, and made up dreams he had never dreamed and little Hanses he had never met. And what happened? Millions of people were out there, all ready and waiting to become neurotic in earnest. And thousands more ready to make money treating them.*
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> Umberto Eco, *Foucault's Pendulum*

In this magazine, Elizabeth Loftus and Melvin Guyer (2002a, b) reviewed a single-case history report that had been hailed as evidence of recovered memory. Psychiatrist David Corwin had captured on videotape the story of the abuse of a six-year-old girl and the recovery, at age seventeen, of her "repressed memories." However, serious doubts were raised when Loftus reviewed the court records and interviewed the girl's mother. Here I review another single-case

history on recovered memory that appeared in the psychological literature. Although my attempts to obtain the facts were less than successful, the adventure provided some lessons about professional credulity and the power of theories that are formed by personal experience.

A Case History Report of Repressed Memory

In 1997, Bertram Karon and Anmarie Widener published an article in *Professional Psychology: Research and Practice* entitled "Repressed memories and World War II: Lest we forget!" In their article, the authors claimed that there were "literally hundreds of documented battlefield neuroses that involved the repression of traumatic combat experiences" and that professionals who worked in the Veterans Administration hospitals (now Veterans Affairs hospitals) after WWII frequently saw such patients.

Karon and Widener then described what they identified as a typical combat hysterical neurosis. In their example, a psychoanalytic psychologist identified as Edward Karon[1] treated a veteran with a hysterical paralysis for six months in twice-weekly sessions. At the end of this period, the patient brought his therapist a newspaper clipping that presumably dealt with an airplane crash in which he and the pilot had been injured. The patient reported that he had been a tail gunner in a two-man bomber, selected because he was small enough to fit into the cramped tail gunner's turret. The pilot, however, was over six feet tall and weighed over 200 pounds. Returning from a mission, the patient said that six of the planes in their squadron crashed during landing, raising the suspicion of sabotage.

Because the runway was littered with wreckage, the patient's plane was forced to land in a field. The tail gunner broke his arm, while the pilot broke both legs and was unconscious. Rescuers refused to approach the burning plane because its fuel was ready to explode. However, with his one good arm, the patient managed to drag the pilot, inch by inch, away from the plane. Although his broken arm subsequently healed, his other arm was thereafter paralyzed. Furthermore, he had no conscious memory of the crash or of saving his friend. He was reported to have repressed it.

After recovering his memory in an emotional therapy session, the patient regained partial movement of his paralyzed arm for the first time. Unfortunately, the secondary gains from this paralyzed arm were not sufficiently resolved for him to return to work until after another year of psychoanalytic psychotherapy.[2] The authors concluded that current controversies concerning repressed memories "are always discussed without reference to this well-documented body of data." They encouraged mental health professionals to "remember their past in order to be effective in the real world." In ways they did not intend, this case history sparked many memories for me because I was well acquainted with stories like these and this style of therapy.

A Search for More Information

Events in war are sometimes stranger than fiction. I know, because in my twenty-five years as a Veterans Affairs psychologist I checked the records of nearly every patient who, like this tail gunner, asserted improbable and self-aggrandizing claims. Time and again the stories turned out to be bogus.[3] Students and colleagues of mine quickly learned not to present a report like that of Karon and Widener's without first obtaining some verification.

The purpose of checking a veteran's story, of course, is not directed at catching lies but at identifying and treating the proper problem. For example, was this man's arm paralyzed at the time of his discharge, and did he receive a Purple Heart? Was he receiving a service-connected disability pension for his symptom? Maybe the war story provided an explanation for his marital and occupational problems. These questions could be answered by consulting the patient's C-file (claim file) or his DD-214.[4] Also, when and where was the newspaper article written? Whether the therapist is a psychoanalyst or a behaviorist, such critical details should always be checked against outside records. Nevertheless, these simple facts are almost never verified, a point I return to later.

I wondered as well what documents were available to Karon for his reconstruction of this case. I believed that the author understood that he would be obligated to provide such information because the Ethical Principles of Psychologists (1992) state that "After research results are published, psychologists do not withhold the data on which their conclusions are based from other competent professionals who seek to verify the substantive claims through reanalysis. . . ."

Thus, in November 1998, I wrote to Dr. Patrick DeLeon, then editor of *Professional Psychology* to ask his assistance. My letter was directed to him because Pendergrast, in preparing a response (1998), had repeatedly made specific requests for documentation, which Karon ignored.[5] My first mailing to DeLeon went unheeded, but he responded to my second request by saying 1) that he thought my first letter was merely a "FYI," needing no reply; 2) that I should write directly to Karon; and 3) that he believed that the ethical code about sharing data applied only to "empirical data." I disagreed about the empirical data limitation on the grounds that the spirit of the code has always been to promote the science of psychology by allowing open examination of "substantive claims," not merely to recheck t-tests.

Subsequently I wrote to Karon. After he failed to respond to my second request, I provided all my correspondence to the Ethics Office of the American Psychological Association for an opinion. Dr. Dolph M. Printz, the acting director of the Office of Ethics, responded by saying that Dr. Gary R. VandenBos was quite familiar with my concerns, and he had summarized his knowledge of the issues in an enclosed memorandum. Printz trusted that the careful review would assure me "that no further action is indicated in this matter."

Surprisingly, the enclosed memorandum by VandenBos was merely a discussion of airplanes. This was clearly not my primary concern and was mentioned only parenthetically in the last paragraph of my letter.

The airplane issue had been raised by software engineer James Giglio, in one of the four responses to the Karon article. Giglio (1998) claimed that no such airplane as the one

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described in the article was ever flown in the European theatre of war, namely a two-man bomber with a tail gunner in a separate tail turret. I wrote Giglio after I read his article, and he provided me with copies of his correspondence with Karon and Widener. Both kept insisting that he was wrong. Widener finally said that she was glad the veteran was no longer around to read Giglio's misguided comments that "completely discounted his experience as a soldier and patriot of this country and of democracy." Karon had also suggested several planes, which Giglio showed as not meeting their criteria. Karon finally insisted that the *Rand McNally Encyclopedia of Military Aircraft* (Angelucci 1981) contained bombers that qualified. Giglio then asked for specific page numbers because he found nothing that fit. When Karon responded, "I do not have time to teach you how to read," their correspondence ended.[6]

The authors' inability to name an aircraft that fit the patient's description seriously damaged the credibility of their story. Yet in Karon and Widener's (1998) response to the critiques of their article, they said that when they informed Giglio about qualifying planes "he then tried to become technical." Even more damaging, they still failed to mention the name of any specific aircraft that they believed might qualify. And although they never acknowledged their article's factual deficiencies, they nonetheless vigorously defended the truth of their story.

Strangely, the VandenBos memorandum focused exclusively on the airplane issue. He said that he had formally sought input from the editor of a WWII aviation magazine who provided several examples: the Mosquito A-20a and A-26, the Douglas SBD Dauntless, the Curtis SB2C Helldiver, the British deHavilland Mosquito, the Douglas A-20 Havoc, the Douglas A-26B Invader, and the Bristol Beaufighter. However, Giglio had already pointed out why these specific planes failed to meet the criteria. The Mosquito A-20 and A-26 did not have a separate tail gunner; the Douglas SBD Dauntless and Curtiss Helldiver were carrier-based dive bombers deployed exclusively in the Pacific; the British deHavilland Mosquito, Douglas A-20 Havoc, and Douglas A-26B Invader each had no tail gunner or

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tail turret; the British Beaufighter was a night fighter, not a bomber, and the only models with separate rear-facing turrets (not in the tail) were non-operational prototypes.

VandenBos opined that any distortions of the patient's memory were a "side detail" and not the essential determinant of accuracy and validity of the clinical discussion. Memory distortion was the issue, and it was difficult for me to dismiss as "side detail" the obvious importance of investigating the patient's service record, clinical treatment notes, and any other data that could "verify the substantive claims" of the article. Then I discovered that VandenBos had co-authored a book with Karon. VanderBos was caught in a conflict of interest. Any hope of finding the facts behind this case were now blocked, and it was clear that many issues remained unresolved.

We applauded each theory knowing that next month our fickle devotion would be overwhelmed by a new series of fascinating case histories.

Boiling Controversy

About a year after the article appeared, *Professional Psychology* published four critical reviews and a response by Karon and Widener. The Giglio article has already been discussed. The review by Lilienfeld and Loftus (1998) was about twice as long as the original Karon article because the authors reviewed a broad spectrum of research concerning the evidence for repression, the role of hypnosis and sodium pentathol in the recovery of memories, problems with the specific case example, and the appropriate use of single case-history reports. Piper (1998) focused on the problem of definitions that confuse discussions of repression, and he also reviewed many of the papers cited by Karon and Widener that they believed supported the notion of repression and amnesia. Finally, the article by Pendergrast (1998) described many examples of recovered war traumas that were false.

The response by Karon and Widener (1998) reflects the bitter divide that infects the issue of repressed memories. They began their article with another case history—this one about a rape. "Would any serious clinician tell her she is lying because there is no such thing as repression?" These reviewers, they charge, are dismissing all WW II patients who suffered trauma and repression as malingerers.

The only point of their article, they insist, was to show that repression exists. "Every psychodynamic therapist sees it. The only way he or she could not see it is by assuming that what the patient says are lies." Although they put up a brave fight over the research, the bottom line for Karon and Widener was that clinicians know repression exists, and "psychologists who dispute the conclusive existence of repression do not do therapy." They implied that those who deny repression are academics who make money by testifying for the defense in court cases, and they agreed with famous attorney Alan Dershowitz when he stated, "The defense has no obligation to tell the truth."

The only hint of a concession in the Karon and Widener article was an acknowledgment that hypnosis and pentathol procedures can be leading and suggestive. Further, "Remembered events may or may not be literally true," but then, "People in or out of therapy have memories of events that never occur as well as memories of events that did occur, but this fact has nothing to do with our article." This admission, it seems to me, suggests the possibility of a *mistaken story by a tail gunner. I can think* of several options other than lying and malingering to explain the onset of hysterical symptoms and recovered memories. They were the ones who brought up the patient's secondary gain—a mark of malingering. Why does a skeptical attitude about repression evoke such distress in some therapists?

Remembering the Lessons

I agree with Karon that the lessons of WWII seem to have been forgotten but "need to be remembered in order for therapists to be effective in the real world." He was also correct in stating that few living clinical psychologists were working in the VA in the 1940s. However, my generation was trained by them. For example, I interacted several times with Jack Watkins who was at the Portland Veterans Administration before moving to the University of Montana where he continued his work in hypnosis and in the multiple personality disorder movement. Further, in 1974, I was president of the Portland Academy of Hypnosis, where month after month speakers shared dramatic case histories that demonstrated the "truth" of their particular theories.

These therapists promoted a vast array of explanations for the development of symptoms. They focused on childhood events, anniversary reactions, blocked emotions, sexual issues, double binds, internal conflicts, hidden trauma, and, of course, repressed memories. We applauded each theory knowing that *next month our fickle devotion would be overwhelmed by a* new series of fascinating case histories. Why did each therapist have a different explanation about the cause of symptoms?

In 1784, the French commission investigating mesmerism found that subjects appeared to know when and where they should have a convulsion only if the mesmerist was present to provide the cues. From the very beginning, patients unwittingly confirmed the theories of their therapists. For example, Zerffi (1871) illustrated the extent of this problem when he said, "Hundreds of trustworthy witnesses have asserted facts which we cannot understand" (p. 67), namely that somnambulists exhibit clairvoyant powers. For example, Grimes (1850) noted that a phrenologist could ask a mesmerized subject to identify the part of her brain where she kept secrets, and she would place her finger exactly on the organ of Secretiveness. Similarly, she could identify other regions of emotions without any understanding of phrenological science. Then, Grimes discovered that phrenologists with different cranial maps obtained information from subjects that confirmed their own individual theories. He concluded: "When the subject, the operator, and all concerned, believe in any peculiar notion, the experiments

will not contradict that notion, but will confirm it, however absurd it may be" (p. 209).

The French neurologist Jean-Martin Charcot confirmed his own theories in a similar manner when he studied hysteria using hypnosis, a process described as "one of the most significant misunderstandings in the entire history of medicine" (Webster 1995, p. 72). Charcot was Freud's most significant mentor, and this problematic methodology was passed on to the generation of psychiatrists who were convinced that the conversion disorders of WW I servicemen were caused by repressed battle trauma. Like Karon's patient, they were often treated with hypnotic abreaction in which the patient was expected to re-live the moment of trauma with unrestrained emotions. They believed that memories revealed during abreaction were completely true to the original experience, and if not, for those who wondered, the process itself was probably necessary for healing.

For example, Hadfield (1940) believed that most of the soldiers with traumatic neuroses had repressed experiences of being buried or blasted by an explosion. He used hypno-analysis to recover these memories, although sometimes "considerable patience and persistence are required to recover the experience" (p. 142). In such cases, he recommended telling the patient that he will not leave the room until he has recovered the experience. "Such persistence nearly always succeeds."

But from WWII on, the number of psychotherapeutic strategies exploded. This was also true for hypnotic interventions, and many of those innovators traveled through the informal speaking circuit of hypnosis societies that I mentioned above. Martin Orne (1959) provided some insight into why this proliferation was happening. Through a series of diabolically clever experiments, he showed that the hypnotic interaction is such a powerful experience for therapist and subject that both remain unaware of how certain implicit cues guide their process. The subject integrates the expectations of the hypnotist in an attempt to be cooperative, while modifying his own story to fit that expectation. Of course, in some situations the patient's story might be true. However, confabulated reports can be "extremely deceiving, as they represent a subjectively real situation, and, therefore, are produced with complete sincerity" (Orne 1951, 221).

Unaware of how much they are influencing each other, both therapist and subject become convinced that *their* theory is true, with the result that they will likely come to view research as contrived or irrelevant to their dynamic experience. Checking the facts seems irrelevant, even confrontational or counter-therapeutic. This powerful subjective experience can lead both parties into false beliefs (Pankratz 2002).

During the Vietnam war, conversion disorders were seldom encountered as repressed memories, and abreactive treatments became a quaint historical artifact. The effects of trauma were now expressed as symptoms of avoidance and intrusion, with flashbacks as a marker.[7] Because this war was unpopular, some suggested that most who participated would have symptoms independent of any constitutional vulnerability—if not now, then delayed. Posttraumatic stress disorder (PTSD) entered the diagnostic manual as a natural adaptation to extraordinary adverse situations (Yehuda et al. 1995).

In 1983, Landy Sparr and I were the first to show how easily this new disorder was feigned. However, PTSD became a wildly popular research enterprise. But in their enthusiasm, most researchers failed to check their subjects' claims or consider more mundane explanations for their symptoms.[8] Like patients who told their therapists what they wanted to hear, research subjects validated experimenters' hypotheses (Orne 1962).

The majority of people exposed to toxic events do not experience any long-term disorder, and delayed responses are extremely rare.

During the twenty years that I have refereed papers submitted to the *American Journal of Psychiatry*, I discovered that many authors merely gathered evidence for what they believed was true about symptoms and the underlying trauma. Fortunately, editors usually understood my skepticism, but it was of great help when Southwick and colleagues (1997) showed that the memories of veterans of Operation Desert Storm were highly inconsistent when questioned one month after combat and then again two years later. Most disturbing was the amplification of recall of traumatic events. Subjects changed their reports to say that they had seen others killed or wounded, that their unit had been ambushed, or that they had encountered booby traps or mines. The authors concluded that "If memories of combat are inconsistent, then the relationship between PTSD and combat exposure would be a tenuous one." An accompanying editorial frankly admitted that no one now knows what posttraumatic stress disorder really is (Hales and Zatzick 1997).

But careful research testing competing explanations has shown us how far we have drifted off course. The vast majority of people exposed to toxic events do not subsequently experience any long-term disorder, and delayed responses are extremely rare. Both children and adults, it turns out, are amazingly resilient in the long run to trauma and unfavorable environments (Bowman 1997; Masten 2001). Pre-existing personal vulnerabilities are more predictive of outcome than an event, just as the DSM-I suggested (Yehuda et al. 1995). Finally, B.G. Burkett and Glenna Whitley (1998) provided compelling evidence that Vietnam veterans are better educated, have a lower suicide rate, have a higher employment record, are underrepresented in prison populations, and have a lower homelessness rate than those who did not serve. They suggested that the VA is not treating posttraumatic stress disorder; they are teaching it.

Conclusions

In 1781, Mesmer fled Paris in disappointment and fury because the commission appointed to investigate him was not interested in the personal experiences of his patients but in whether there was evidence for his underlying assumption of animal magnetism. In the 1880s, Charcot ordered doubters out of his hospital

when they questioned the value of his Tuesday lectures. In the twentieth century, psychiatrists disdained the idea of checking the reality of abreactions and self-reported trauma. As a result, posttraumatic stress disorder disability pensions may now cost taxpayers $2 billion a year, and we must face the possibility that two decades of posttraumatic stress disorder research, all based on dubious self-reports, may be useless.

In the single-case history report investigated by Loftus, small inconsistencies were ignored by professionals who were overwhelmingly convinced by the emotional response of the subject. When Loftus looked for all the facts, she became the object of some serious harassment (Tavris 2002). James Giglio was accused of being unpatriotic when he asked for information, and the American Psychological Association would rather talk to aviation experts than acknowledge whether or not any documents support a repressed memory report.[9]

From these generations of neglected critical questioning emerged an eagerness to treat recovered memories, multiple personality disorders, and traumas of every sort. The disheartening news is that we have yet to discover an effective treatment for those who really suffer from chronic posttraumatic stress (Shalev et al. 1996) or from the acute effects of trauma. Litz and colleagues (2002) reviewed six studies of early interventions for acute trauma that they judged as having sound methodology. In all instances, psychological debriefing failed to promote change to a greater degree than no intervention at all, and in two studies the symptoms of treated victims became worse over time. While society demands that mental health professions help, sufferers are likely to be better off relying on their own natural support systems.

I believe psychologists have a responsibility to provide safe and effective treatments to those who use our services. Karon and I agree on one thing: Mental health professionals need to remember their past in order to be effective in the real world.

Notes

1. Bertram Karon told Beth Loftus that Edward was his brother who had died about twenty years previously.

2. I published a single-case report describing two sessions of hypnosis to treat a similar hysterical paralysis (Pankratz 1979). My point was that a face-saving strategy can avoid a struggle over the etiology of symptoms, and it is not necessary that the paradigm fit the facts to be effective.

3. See, for example, Pankratz 1990, 1998; Pankratz, Hickam, and Toth 1989; Pankratz and Jackson 1994; Pankratz and Kofoed 1988; Pankratz and Lipkin 1978; and Pankratz and McCarthy 1986.

4. The DD-214 is the veteran's discharge document that provides a general review of the individual's military history. The DD-214 is now so commonly forged, however, that it should no longer be considered a reliable document.

5. Interested readers can obtain a copy of this correspondence from Mr. Pendergrast at markp@nasw.org.

6. Interested readers can obtain a copy of this correspondence from Mr. Giglio at jgiglio@nova.umuc.edu.

7. Jones, et al. (in press) examined symptoms of UK servicemen from 1854 to the present. They concluded that symptoms of stress have changed dramatically over time and that PTSD (as described in the diagnostic manual) is a culture-bound syndrome.

8. My favorite example is from the National Vietnam Veteran Readjustment Study (NVVRS), research that consumed four years and $9 million (Kulka et al. 1988). Six women in the study claimed that their stress was caused by being a prisoner of war. Not one of the many researchers involved in the study apparently realized that no American military woman ever became a POW in Vietnam.

9. The American Psychological Association recently was accused of backing away from some controversial scientific findings. To their credit, they devoted an issue of the *American Psychologist* to the whole affair (see Lilienfeld 2002).

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'Premenstrual Dysphoric Disorder' and 'Premenstrual Syndrome' Myths

Neither Premenstrual Dysphoric Disorder (PMDD) nor Premenstrual Syndrome (PMS) is a medical or mental disorder or illness. Premenstrual changes are a normal part of life. Belief in PMS/PMDD is a result of conditioning, not biological processes, and best treated as such.

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Premenstrual dysphoric disorder (PMDD) is not a disorder, illness, or disease, but an extreme point on the continuum of premenstrual tension. Nor is "premenstrual syndrome" (PMS) an illness or disorder. Premenstrual physical changes related to the menstrual cycle are a normal part of female life.

Emotional and behavioral problems associated with menstruation in some women are a result of basic conditioning processes—reinforcement and modeling. Expressions of PMS symptoms are negatively reinforced when they result in sick days from work or school or allow the "sufferer" to escape or avoid anxiety-provoking or unpleasant social situations. PMS provides an excuse for what would otherwise be

unacceptable behavior. PMS behaviors are positively reinforced when expression of symptoms result in social comfort or medical care. PMS symptoms develop when they are modeled by others as expected patterns of female behavior. Cognitive-behavioral therapy, nutrition, and exercise, are the most rational and the most effective treatment for PMS symptoms. Drugs given for PMS produce many side effects (e.g., anxiety) that are the same as reported "symptoms" of PMS.

Physiology of Menstruation

The body's hormone interactions are responsible for the menstrual cycle. Seven to ten days prior to the onset of menstruation, many women report symptoms of "premenstrual syndrome" (PMS). "The most common physical complaints include breast tenderness, bloating, weight gain, headache, and fatigue. Common emotional and mental symptoms include irritability, difficulty concentrating, moodiness, depressed feeling, anxiety, anger, and food cravings" (Premenstrual Syndrome 2001). The psychological community, the medical-psychiatric community, and feminists all have varying opinions on the validity of PMS and PMDD as *mental or physical illnesses.* The consensus among psychiatrists, who consider PMS/PMDD to be a valid disorder, is that drugs should be the first line of treatment. However, neither PMS nor PMDD has been scientifically established as an illness.

A Brief History of PMS

"The notion that women's reproductive systems affect their lives is ancient," notes Linda Brannon in *Gender: Psychological Perspectives* (Brannon 1996, 62). Historically and across cultures, as recently portrayed in Anita Diamant's award-winning novel *The Red Tent,* (2001) menstruation was celebrated: "how my mothers celebrated the new moon [menstruation] with cakes and songs and stories, leaving ill will outside" (133). Menstruation was seen as a reconfirmation of "womanhood," and eagerly looked forward to by many women—"a gift to women that is not known among men, and this is the secret of blood . . . to men this is flux and distemper, bother and pain. They imagine we suffer and consider themselves lucky. We do not disabuse them. . . . In the red tent, the truth is known . . . women give thanks—for repose and restoration, for the knowledge that life comes from between our legs, and that life costs blood" (158). Once, menstruation was seen as the essential part of life that it is. How did normal menstrual symptoms come to be viewed by some as an illness?

In *Psychobbable and Biobunk,* Carol Tavris concludes that PMDD is "just ancient superstition in pompous new jargon" (2001). The problem of whether or not severe PMS—PMDD—is a real mental disorder is actually a particular case of the general problem of when any set of conditions constitutes a mental disorder. In *Creating Mental Illness* (2002) Rutgers sociologist Allan Horwitz argues that "psychiatrists, epidemiologist, and clinicians simply

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accept as mental disorders whatever conditions the DSM [a psychological diagnostic manual] lists. They do not ask how these conditions came to be regarded as mental disorders . . . there is no reason to accept that any particular group of symptoms represents a valid form of mental disorder" (pp. 19–20).

"Premenstrual tension" was first described in 1931. In 1953 Katherine Dalton introduced the term "premenstrual syndrome," and in subsequent years promoted and researched PMS (see Dalton 1987). The list of PMS symptoms, some directly contradicting others, has grown to over 150. Dalton notes that "The symptoms themselves are commonplace and also occur with great frequency in men, children, and postmenopausal women" (1987, 717). How can this vast group of common "symptoms" that occur in all people represent a valid mental disorder?

Tavris argues that PMS is a "manufactured" problem (e.g., a fiction). To support this claim Tavris notes that although described in 1931, it wasn't until 1964 that the first article on PMS appeared in a medical journal, and 1966 for the first psychological journal article. But by 1989 there were 305 medical journal articles and 120 psychological journal articles on PMS (Tavris 1992, 140). Women have always ovulated and menstruated, but research on PMS "erupted" in the 1970s, according to Tavris, because "When women's participation in the labor force is seen as a threat instead of a necessity, menstruation becomes a liability" (141). Along the way, as noted by Tavris, PMS was "coopted" by biomedical researchers—"The big money, the big grants" (141). Indeed, today the pharmaceutical companies are the biggest promoters of PMS/ PMDD. Do real medical problems need promoting?

In 1987, the American Psychiatric Association's *Diagnostic and Statistical Manual of Mental Disorders*, third edition-revised (*DSM-III-R*), labeled those with severe PMS symptoms as meeting the criteria for "late luteal phase dysphoric disorder." In 1994 the *DSM-IV* task force (literally a group of psychiatrists that decide, not by scientific data, but by consensus what to call a "disorder") put late luteal phase dysphoric disorder in the appendix as "premenstrual dysphoric disorder" (PMDD) under "disorders needing further study." This inclusion legitimized PMDD for women, the medical community, and the insurance industry. It wasn't long until Prozac, repackaged in pretty purplish pills and called "Sarafem," was being readily offered to women for mentioning "commonplace symptoms."

For example, when Melissa (the second author) casually asked her doctor how a new birth control pill was with "PMS symptoms," before she could finish her sentence the doctor offered her Sarafem. Melissa did not say she had severe symptoms or was depressed. The doctor didn't ask what symptoms Melissa was referring to. He simply offered her a powerful pharmaceutical. This occurrence is not unique. Like millions of other women, Melissa's sister was given a free sample of Sarafem from a different doctor for a different "problem"—she had a baby.

Through observational learning and reinforcement women are taught that premenstrual symptoms are problematic. if girls are taught that menstruation is normal, then as women they are unlikely to perceive the symptoms as problematic.

Conditioning of PMS

Reports of subjectively experienced premenstrual pain are not a product of actual physical severity, but of basic conditioning processes—reinforcement and observational learning. In an article "Modeling and reinforcement of the sick role during childhood predicts adult illness behavior," the authors, led by William E. Whitehead of the Division of Digestive Diseases of the University of North Carolina, studied 382 women and found that "subjects were most likely to report the symptoms and to take disability days for the symptoms that their parent reinforced and modeled" (Whitehead et al. 1994, 548). Specifically: "Childhood reinforcement and modeling of menstrual illness behavior had a significant influence on the number of physician visits for menstrual symptoms" (547). Similarly, Canadian researchers Melanie Thompson and Mary Glick found that among college students: "Consistent with previous research, care-seekers reported more reinforcement for adolescent menstrual illness behaviors than non-care-seekers. Care-seekers also reported their symptoms as more serious and more difficult to ignore. The perceived seriousness and severity of symptoms were both correlated with reinforcement for adolescent menstrual symptoms" (2000, 137).

Mexican researchers Luisa Marvan and Claudia Escobedo (1999) studied PMS with eighty-six healthy Mexican women who had no higher than a sixth grade education. A "Menstrual Distress Questionnaire" (MDQ) was given to all women one week after menstruation. Then half the women were shown a video describing the menstrual cycle. The other women were shown a video describing PMS and its negative consequences. All the women were again given the MDQ after their next menstruation. There were no changes in the women who simply learned about the menstrual cycle, but the women who watched the PMS video reported more severe premenstrual symptoms after watching the tape. Would watching a video about measles produce measles?

These results show that viewing premenstrual symptoms as problematic *is not the result of physical conditions.* Through

observational learning and reinforcement *women are taught* that premenstrual symptoms are problematic. The converse is that if girls are taught that menstruation is normal—nonproblematic, then as women they are unlikely to perceive the symptoms as problematic.

Studies that ask people to chart their moods and physical state daily for extended periods of time fail to find any correlation between "symptoms" of PMS or PMDD and impending menstruation.

Misattribution and Illusionary Correlations

The more salient different stimuli or events are, the more likely they are to be associated. Therefore, once women come to believe they "have PMS," other salient physical states, emotional states, and behaviors are likely to be believed to be caused by PMS. A University of Texas-Houston Department of Psychiatry and Behavioral Sciences study (Dougherty et al. 1998) found that aggression did not vary across the menstrual cycle. They did find that women with high MDQ scores (PMS) were almost twice as aggressive as women without PMS during *all* menstrual cycle phases (premenstrual, menstrual, mid-follicular, and ovulation).

The Houston study shows that the link between aggression and premenstruation is an illusion. Aggressive women are aggressive across the cycle, believing they have PMS just gives them an excuse for much of their aggression. Indeed, an earlier study of violent women offenders found that only 29 percent committed their crimes during the premenstrual phase (71 percent were committed during other phases), indicating menstrual phase has no effect on the likelihood of a woman behaving violently (d'Orban and Dalton 1980). The premenstrual phase is not a cause of aggression.

Studies that find relationships between premenstrual phase and inappropriate behaviors or psychological problems are retrospective self-report surveys, often with the intent of the study known to the participants. Such findings suggest PMS is an illusion due to women's (learned) expectations and (mis)attributions, not biology. Other studies (such as Aubuchon and Calhoun 1985) find that symptoms are exaggerated if women believe menstruation is the focus of the study.

But when the purpose of a PMS study is disguised there is little evidence of PMS. For example, when Maria Marvan and Sandra Cortes-Iniestra compared disguised-purpose daily reports with retrospective reports of premenstrual changes they found much higher retrospective reports of premenstrual change than actually occurred according to the daily reports. The researchers concluded that "many women have a misperception about the meaning of PMS: consequently, they amplify their premenstrual changes in recall, reflecting women's cultural stereotypes rather than their actual experiences" (2001, 276).

In addition to cultural stereotypes, experimentally produced biases ("*expectancy effects*") *cause changes in reported* premenstrual symptoms. There are several questionnaires other than the MDQ that evaluate premenstrual changes, and the amount of reported distress depends on the questionnaire given. Indeed, compared to women who first complete a MDQ and then a Menstrual Attitude Questionnaire (MAQ), women who first complete a Menstrual Joy Questionnaire (MJQ) report more positive cyclic changes on the MAQ (Chrisler et al. 1994). The MJQ asks subjects to evaluate the experience of positive symptoms (such as increased sexual desire, sense of euphoria, and feelings of power) the week preceding menstruation. Menstruation is a fact, but whether it is interpreted as joyful or distressful depends on what one is taught to expect (see also Diamant 1997).

Studies that do not inform the participants that the study is looking at PMS but simply ask people to chart their moods and physical state daily for extended periods of time fail to find any correlation between "symptoms" of PMS or PMDD and impending menstruation (Mcfarland, Ross, and DeCourville 1989). Studies *find that both men and women* regularly cycle though mood changes, on both daily and monthly cycles (McFarlane et al. 1988).

Treating PMS and PMDD

Irritability, anxiety, and depression associated with the onset of menstruation is very real for the woman even if the suffering is the result of conditioning and misattribution. PMS distress deserves effective treatment. To dismiss PMS as "all in you head" would be as dismissive as telling a depressed person "don't worry, be happy." How then should PMS be treated?

Exercise

Water retention and abdominal discomfort may occur premenstrually, and decreasing the physical symptoms through regular exercise in turn decreases the behavioral and emotional symptoms. Regular exercise improves psychological functioning for *all* individuals regardless of the "diagnosis" of the individual suffering. A research review by Australian researchers Byrne and Byrne (1993) found that exercise was successful in producing antidepressant, anti-anxiety, and mood-enhancing effects across mood disorders. A team of Duke University medical researchers (Babyak et al. 2000) recently found that

exercise was as effective as antidepressant medications such as Prozac in treating major depressive disorder at six months, and at ten months fewer exercisers relapsed into depression than drug takers. That is, exercise is more effective than SSRI drugs in treating major depression. Exercise is equally effective in treating PMS/PMDD. Strength training produces general improvement in premenstrual symptoms (e.g., Steege and Blumenthal 1993). Aerobic exercise produces even greater improvements, and is "protective" against both physical symptoms such as water retention and psychological symptoms including dysphoric mood (depression) anxiety, pain, and functional impairment (Bibi 1996; Stoddard 1999)

The protective nature of exercise against PMS provides a clue as to why premenstrual tension became a problem. Since the early 1900s, people in the "developed world" have become increasingly sedentary. Obesity is at epidemic levels. Before modern labor-saving devices, living required regular exercise. As a result, problems such as obesity and PMS were rare. Therefore it is not surprising that women who exercise have significantly fewer PMS symptoms than non-exercisers (Johnson, Carr-Nangle, and Bergeron 1995).

Nutrition

Corresponding to the decrease in the amount of exercise required by daily life there has been an increase in food availability and food consumption—particularly consumption of high fat and high sugar foods. An increase in fat consumption and a decrease in low-fat vegetable consumption contributes to PMS symptomology. Improving nutritional habits effectively decreases premenstrual distress.

Because "anecdotal reports indicate that a low-fat, vegetarian diet might reduce menstrual pain," and because several research findings have established that "plant-based and vegetarian diets increase serum concentration of sex-hormone binding globulin, which binds and inactivates estrogens" (245), Georgetown University researchers (Barnard, et al. 2000) studied the effects of a vegetarian diet on dysmenorrhea (painful menstruation) and premenstrual symptoms. The vegetarian diet resulted in weight loss, decreased dysmenorrhea, decreases in pain and water retention, and other premenstrual symptoms. Following the Federal Drug Administration's recommended diet of five servings of fruits and vegetables and minimal animal fat would result in greatly improved menstrual health including a decrease in PMS symptoms.

Cognitive Behavioral Therapy

Although effective, exercise and diet alone may not address the misattributions that problematic behaviors (e.g., aggression) and emotions are caused by premenstrual tension. Fortunately, education and therapy directed at changing these beliefs is effective (Christensen and Oei 1995). Cognitive therapy that includes diet and exercise recommendations has shown to be an effective treatment for PMS. Fiona Blake and her coworkers (1998) treated women diagnosed with PMS with cognitive therapy that included: treatment goals, education, engaging in new behaviors for homework, and gaining new skills (p. 311). Results showed that "cognitive therapy for premenstrual syndrome (PMS) was found to be associated with significant improvements in a range of measures, which included measures of PMS symptoms, associated impairments, and depression . . . there was no sign of any tendency to lose the gains made" (316).

Dissertation work by Susan Koons indicates that the best treatment for PMS may involve a combination of exercise, diet, and cognitive-behavioral therapy (CBT). When women who were diagnosed with severe PMDD completed a CBT treatment package that included diet and exercise, the scheduling of pleasant events, and stress management, symptoms were alleviated and gains were maintained at six month follow up (Koons 1999).

Drug "Treatment"

Despite the fact that exercise, dietary changes, and CBT have been shown to be equally effective as (or more effective than) drugs in treating mood disorders including PMDD, drugs are considered to be "the first line treatment option" (Pearlstein and Steiner 2000) by psychiatrists. Clearly, if womens' best interest was the primary consideration then the suggestions mentioned thus far would be the first line of treatment. But psychiatrists are medical doctors, trained to give diagnosis and treat problems with drugs. Thus if the disorder is diagnosed, then it is only logical that doctors would to look to drugs to treat the illness. However, the data suggests that drug treatment is short-sighted.

The first drug approved specifically for PMDD by the FDA is Sarafem (previously marketed as Prozac). Prozac is a selective serotonin reuptake inhibitor (SSRI). SSRIs work by preventing the reuptake of the neurotransmitter molecule serotonin by the neuron that released it. Preventing reuptake increases serotonin's concentration in the synapse between communicating neurons and thus increases neuronal communication. Prozac was developed for depression. SSRIs were *not* developed for, or assumed to have anything to do with, menstruation. With the impending expiration for the patent on Prozac, Eli Lilly's multibillion-dollar drug, the color of the pill was changed and on July 6, 2000, the FDA approved the "new" drug Sarafem for PMDD. As listed on the "information for the patient" insert included in packets of Sarafem, some of the side effects include "tiredness, upset stomach, nervousness, dizziness, and difficulty concentrating." These side effects are among the very "symptoms" of PMDD!

One of the impetuses for Sarafem/Prozac being approved by the FDA was a 1995 study (Steiner, Steinberg, Stewart, et. al.), funded by Eli Lilly, that found that for 52 percent of sufferers drug treatment produced "moderate improvement." Obviously, yet ignored, this means the drug did *not* help (or only slightly helped) 48 percent. The efficacy of Prozac also decreased considerably after four months.

Furthermore, taking drugs for PMS is likely to produce misattributions—e.g., "If I'm feeling good now, it must be because I'm on Sarafem. It couldn't be due to my own actions and of those around me. I'm not aggressive now because I'm

on drugs." Such misattributions are likely to foster further drug dependence—e.g., "I need Sarafem or I'll be a real bitch." The side effects of SSRIs such as Sarafem are detrimental, whereas the side effects of improved diet and exercise are beneficial.

In contrast to drug treatment, behavioral approaches including lifestyle changes (involving diet, exercise, social skills, and education) directly confront the sources of PMS (poor diet, lack of exercise, poor social skills, misattributions, and ignorance about menstruation). While the initial costs of changing one's lifestyle may be higher than simple pill popping, the results are much greater (e.g., Babyak et al. 2000). Diana Taylor (1999) of the University of California at San Francisco investigated the effectiveness of a symptom management intervention involving self-monitoring/regulation (diet regulation, vitamin supplementation, and exercise), and self/environment modification ("lifestyle alterations, . . . change in daily activities, and interpersonal competency training") with ninety-one women with severe PMS. Treatment produced "dramatic reductions in premenstrual symptom severity (75–85 percent)" (507). Results were maintained or enhanced at follow up. Taylor (1999) compared these results with those of drug treatment:

> a 75–85 percent reduction in PMS severity in women [results from] using a combination of dietary, exercise, and behavioral, cognitive, and environmental stress management strategies (Goodale et al. 1990; Kirby 1994). When compared to antidepressant drug therapy, the PMS-SMP [Symptom Management Program] was more effective in reducing premenstrual symptom severity and distress than studies testing fluoxetine [Sarafem/Prozac], which demonstrated 40–50 percent improvement in PMS severity, but up to one third of women will discontinue medication use due to drug side effects such as nausea, disturbed sleep, fatigue, and dizziness" (Steiner et al. 1995).

Thus, there is only one rational conclusion concerning treatment for the so called "disorder" of PMS/PMDD: Behavioral, educational, and cognitive-based treatment should be the first line treatment option. Ignorance, convenience, and the profit motive, not women's health, are why drug treatment is advocated.

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It is rare that we feel moved to republish an earlier SKEPTICAL INQUIRER *article. But the myth that a patent commissioner once resigned because "everything that* can *be invented* has *been invented" keeps being uncritically repeated in prominent news outlets. So we thought it would be interesting and useful to reprint Samuel Sass's brief article investigating that claim, "A Patently False Patent Myth," from our Spring 1989 issue. The article has not appeared in any SI anthology. Author Sass has slightly revised one paragraph, and at the end he provides an update.—*EDITOR

A Patently False Patent Myth—Still!

Did a patent official really once resign because he thought nothing was left to invent? Once such myths start they take on a life of their own.

SAMUEL SASS

For close to a century there has periodically appeared in print the story about an official of the U.S. Patent Office who resigned his post because he believed that all possible inventions had already been invented. Some years ago, before I retired as librarian of a General Electric Company division, I was asked by a skeptical scientist to find out what there was to this recurring tale. My research proved to be easier than I had expected. I found that this matter had been investigated as a project of the D.C. Historical Records Survey under the Works Projects Administration. The investigator, Dr. Eber Jeffery, published his findings in the July 1940 *Journal of the Patent Office Society.*

Jeffery found no evidence that any official or employee of

Rob Pudim

the U.S. Patent Office had ever resigned because he thought there was nothing left to invent. However, Jeffery may have found a clue to the origin of the myth. In his 1843 report to Congress, the then-commissioner of the Patent Office, Henry L. Ellsworth, included the following comment: "The advancement of the arts, from year to year, taxes our credulity and seems to presage the arrival of that period when human improvement must end." As Jeffery shows, it's evident from the rest of that report that Commissioner Ellsworth was simply using a bit of rhetorical flourish to emphasize that the number of patents was growing at a great rate. Far from considering inventions at an end, he outlined areas in which he expected patent activity to increase, and it is clear that he was making plans for the future.

When Commissioner Ellsworth did resign in 1845, his letter of resignation certainly gave no indication that he was resigning because he thought there was nothing left for the Patent Office to do. He gave as his reason the pressure of private affairs, and stated, "I wish to express a willingness that others may share public favors and have an opportunity to make greater improvements." He indicated that he would have resigned earlier if it had not been for the need to rebuild after the fire of 1836, which had destroyed the Patent Office building. In any case, the letter of resignation should have put an end to any notion that his comment in the 1843 report was to be taken literally.

Unfortunately, the only words of Commissioner Ellsworth that have lived on are those about the advancement of the arts taxing credulity and presaging the period when human improvement must end. For example, the December 1979 *Saturday Review* contained an article by Paul Dickson titled "It'll Never Fly, Orville: Two Centuries of Embarrassing Predictions." He lists a pageful of "some of the worst wrong-headed predictions." Ellsworth's rhetorical sentence is included with such laughable statements as that said by Napoleon to Robert Fulton: "What sir, you would make a ship sail against the wind and currents by lighting a bonfire under her decks? I pray excuse me. I have no time to listen to such nonsense."

If in the case of Commissioner Ellsworth there was at least a quotation out of context on which the "nothing left to

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invent" story was based, a more recent myth attributing a similar statement to a commissioner who served a half-century later is totally baseless. This news story surfaced in the fall of 1985, when full-page advertisements sponsored by the TRW Corporation appeared in a number of leading periodicals, including *Harper's* and *Business Week*.

These ads had as their theme "The Future Isn't What It Used to Be." They contained photographs of six individuals, ranging from a baseball player to a president of the United States, who had allegedly made wrong predictions. Along with such statements as "Sensible and responsible women do not want to vote," attributed to President Cleveland, and "There is no likelihood man can ever tap the power of the atom," attributed to physicist Robert Millikan, there is a prediction that was supposedly made by Commissioner of the U.S. Patent Office Charles H. Duell. The words attributed to him were: "Everything that can be invented has been invented." The date given was 1899.

Since I was certain that the quotation was spurious, I wrote to the TRW advertising manager to ask its source. In response to my inquiry, I received a letter referring me to two books, although I had specifically asked for the *primary* and not secondary sources. The books were *The Experts Speak*, by Christopher Cerf and Victor Navasky, published in 1984 by Pantheon, and *The Book of Facts and Fallacies*, by Chris Morgan and David Langford, published in 1981 by St. Martin's Press.

When I examined these two volumes I found that the 1981 Morgan and Langford work contained Commissioner Ellsworth's sentence about the advancement of the arts taxing our credulity, although the quote was somewhat garbled. It also contained the following comment by the authors: "We suppose that at just about any period in history one can imagine, the average dim-witted official will have doubted that anything new can be produced; the attitude cropped up again in 1899, when the director of the U.S. Patent Office urged President McKinley to abolish the office, and even the post of director," since "everything that can be invented has been invented." The authors do not give the name of the commissioner whom they call "director," but it was Charles H. Duell who held that office in 1899. They don't offer any documentation to support that alleged statement, and they would have had a tough time finding any.

It's easy enough to prove that Duell was not the "dim-witted official" so glibly referred to. One need only examine his 1899 report, a document of only a few pages, available in any depository library. Far from suggesting to the president that he abolish the Patent Office, Duell quotes the following from McKinley's annual message: "Our future progress and prosperity depend upon our ability to equal, if not surpass, other nations in the enlargement and advance of science, industry and commerce. To invention we must turn as one of the most powerful aids to the accomplishment of such a result." Duell then adds, "May not our inventors hopefully look to the Fifty-sixth Congress for aid and effectual encouragement in improving the American patent system?" Surely these words are not those of some kind of idiot who believes that everything has already been invented. Other information in that report also definitely refutes any such notion. Duell presents statistics showing the growth in the number of patents from 435 in 1837 to 25,527 in 1899. In the one year between 1898 and 1899 there was an increase of about 3,000. It's hardly likely that he would expect a sudden and abrupt ending to patent applications.

The other book cited by the advertising manager of TRW, Inc., *The Experts Speak*, by Cerf and Navasky, offers a key to how myths are perpetuated. This volume, published three years after the Morgan and Langford work, contains the spurious Duell quote, "Everything that can be invented has been invented," and prints it as though it had formed part of the commissioner's 1899 report to President McKinley. However, unlike the earlier work, *The Experts Speak* contains source notes in the back. The source given reads as follows: "Charles H. Duell, quoted from Chris Morgan and David Langford, *Facts and Fallacies* (Exeter, England, Webb & Bower, 1981), p. 64." Unlikely as it is for the head of the U.S. Patent Office to have said something so silly, evidently it did not occur to Cerf and Navasky to question that statement. They simply copied it from the earlier book. One can expect that in the future there will be more copying because it is easier than checking the facts.

The irony is that the subtitle of *The Experts Speak* is "The Definitive Compendium of Authoritative Misinformation." One can only wonder how much more misinformation is contained in this nearly 400-page compendium. On the title page the book is described as a "joint project of the *Nation* magazine and the Institute of Expertology." Whatever this institute may be, on the theory that the *Nation* is a responsible publication, I wrote to Mr. Navasky, who is editor of that magazine and coauthor of the book, to ask if he could tell me where and when Commissioner Duell made the stupid statement attributed to him. I did not receive a reply.

Addendum to Original Article

The earliest appearance of the patent myth in print that I am aware of is the October 16, 1915, issue of *The Scientific American*. It contains the following item: "Someone poring over the old files in the United States Patent Office in Washington the other day found a letter written in 1833 that illustrates the limitations of the human imagination. It was from an old employee of the Patent Office, offering his resignation to the head of the department. His reason was that as everything inventable had been invented the Patent Office would soon be discontinued and there would be no further need of his services. . . ."

As in all "urban legends," the details are vague. Neither "the old employee" who is supposed to have resigned nor the "someone poring over the old files" is identified. The fact is that when the Patent Office burned to the ground in 1836, all records were destroyed so that even if that 1833 letter had ever

A PATENTLY FALSE PATENT MYTH STILL!
Continued on page 48

Wired to the Kitchen Sink

Studying Weird Claims for Fun and Profit

An evaluation of Dr. John Upledger's craniosacral therapy illustrates an exercise proposed for skeptics to develop critical thinking and a better understanding of human psychology.

HARRIET A. HALL

After reading some particularly egregious nonsense, you have probably asked yourself, "How could anyone in his right mind believe that?" There is an answer to your question. In fact, the person who believes the nonsense will usually provide the answer himself if you give him half a chance. Go to the source. Read the believer's account of how he came to believe. He will probably give a clear enough description that you can see where he went wrong. It will give you an insight into human psychology. It will probably also be very entertaining. Here is an example of how it works.

I recently heard of craniosacral therapy. It is a method some osteopathic physicians use to restore health by adjusting

the bones of the skull and sacrum. Anatomists can demonstrate that the skull bones are fused together in adulthood and cannot move. Other fallacies inherent in the therapy are too numerous to list: craniosacral therapy is totally implausible and has been thoroughly debunked elsewhere.

So how could anyone in his right mind believe in it? To find out, I went to the horse's mouth. My local library had a book entitled *Your Inner Physician and You: CranioSacral Therapy and SomatoEmotional Release*, by Dr. John E. Upledger, who is a major proponent of craniosacral therapy. I decided to read his account with as open a mind as I could summon, and give him a fair chance to convince me.

He describes his "eureka" moment. He was assisting a neurosurgeon by holding the dura (membrane surrounding the brain and spinal cord) steady while the surgeon removed a calcified plaque. He wasn't doing a very good job of holding still. The surgeon complained. Most of us would have thought our own muscles were at fault; however, Upledger observed that the dura was fluctuating up and down at about ten cycles per minute, overcoming his attempts to hold it still. Nobody had ever observed this before.

He hypothesized that this "craniosacral rhythm" was intrinsic to human health. Since the cerebrospinal fluid within the dura is in a closed space, the skull bones must move in and out to accommodate the rhythmic changes. The nervous system controls the rest of the body, so if the bones are not moving freely, nerve conduction might be abnormal and health might suffer. Perhaps he had found the basic cause of all disease.

He tried mobilizing the cranial bones through hands-on manipulation, and convinced himself he could *feel* the bones move one-sixteenth of an inch or more. Patients with autism, seizures, cerebral palsy, headaches, dyslexia, colic, asthma, and other diseases reported dramatic improvement. He found that well people treated with monthly adjustments reported more energy, felt happier, and were sick less often.

He felt he had achieved a glimpse into the "core." He states, "I'm not quite sure as yet what the core is all about, but I do know that at times the craniosacral system feels like the entrée into the deepest region of the patient's (and my own) total being. I'm not quite sure as yet what the 'total being' is all about, but it feels like the craniosacral system is where it all comes together, whatever it is."

So far, he had failed to convince me. It seems obvious that he had experienced an illusion (ideomotor activity, as experienced by Ouija boarders and water witchers). His false perception was reinforced when patients seemed to respond to treatment (perhaps from placebo effect, suggestion, hypnosis, a wish to agree with the doctor, the natural tendency of symptoms to improve with time, hands-on massage therapy, or whatever). I found his claims implausible and unbelievable.

This was bad enough, but then he really got carried away. He thought he had found a "cure-all," but apparently it wasn't enough to cure all, since he proceeded to add other treatments to his armamentarium. He discovered *tissue memory*: he found his hands "almost moving by themselves" to certain areas of the patient's body that seemed to contain some sort of memory of an old injury. He could feel heat and pulsations: under his hands these increased, then decreased until the sensations seemed to stop. At that point the patient's pain would subside. The patient sometimes felt an emotion (fear, anger) and at that time or later might remember a forgotten injury.

Dr. John E. Upledger regressed a patient to age two days, where she remembered her grandmother saying she should never have been born; insight caused her symptoms to disappear. He asked to talk to a patient's Inner Physician, who would explain the cause of the illness. One Inner Physician appeared to the patient in the form of a seagull and asked to be addressed as "Mermaid."

He proceeded to discover *energy cysts* (energy from an injury supposedly forms a ball deep in the tissues and stays there until released), *somatoemotional* release (touching the patient and giving permission allows old traumatic memories to surface and ventilate), and *healing energy* (which he transmits from the fingers of one hand to the other hand through the patient's body). Next he tried hypnosis. He regressed a patient to age two days, where she remembered her grandmother saying she should never have been born; insight caused her symptoms to disappear. He asked to talk to a patient's Inner Physician, who would explain the cause of the illness. One Inner Physician appeared to the patient in the form of a seagull and asked to be addressed as "Mermaid." (I am not making this up.)

He found that combinations of therapies worked even better than one at a time. Multiple hands with multiple therapists got results logarithmically faster. He tried therapy in float tanks: it worked wonders. He tried swimming with dolphins: multiple therapists surrounded the patient and were told to silently encourage the dolphins' freedom of choice, and to think that the dolphins were at least equal to them in knowledge and skill, and probably superior. (Perhaps this *was* true.) Dolphins spontaneously came up and touched the *therapist's* back to relieve the *patient's* symptoms. In one case, they reduced a leg-length discrepancy from three inches to one inch. At the same time the therapists' own skills were enhanced in "some non-conscious way."

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Onward and upward, enthusiasm undaunted, he tried sound therapy for an orchestra conductor. Hearing a cello play a concert "A" tone caused back pain; "G" relieved upper and lower back pain, while "B" worked only for his upper back.

He would try anything his intuition suggested to him. He sensed that a patient had excess energy, so he *grounded the patient's big toe to a drainpipe* with copper wire, and—lo and behold—it worked! He successfully applied the same therapy to a woman with sympathetic reflex dystrophy who was in so much pain she would scream when the therapist's hand came within three inches of her body (!?). He had her husband *connect her to the kitchen sink* at home with a thirty-foot copper grounding wire so she could get around the house. (My husband says this sounds like a reasonable way to control a wife, but he may be prejudiced.)

Upledger never reports a failure. Everything seems to work for him. With that kind of reinforcement, how could he doubt? He is an intelligent and educated man and is admirable in many ways. You have to respect his creativity, imagination, and the courage to try unorthodox things in defiance of traditional medical education. New ideas should be encouraged: eventually some will lead to breakthroughs. It is obvious that he believes he has helped patients and patients believe they have been helped.

If the skull bones really could move as much as one-sixteenth of an inch, it would be easy enough to measure and prove it. Unfortunately, Upledger is so convinced his treatments work that he has no motivation to prove the effect is real. He wants to keep on truckin' and to "spread the word." He doesn't pause to reflect that if his hypotheses were demonstrated to be true, it would be a major scientific discovery worthy of a Nobel Prize. He doesn't stop to think that he is subjecting his patients' money, time, health, and perhaps even their lives to unproven treatments.

Evolution has given the human mind a great ability and desire to find patterns everywhere and the motivation to seek explanations. If we can't find meaning, we will invent it. If something seems to work once, we will repeat it with the expectation that it might work again. We have a tendency to jump to conclusions because there is a survival value to deciding quickly. We rely strongly on personal testimonials, because until recently (in an evolutionary sense) that's all we had to go by. As any magician knows, our senses are prey to many kinds of illusions. Illusory perceptions allow us to accomplish things we couldn't otherwise do; for instance, our brains fill in the blind spot in our visual field. All these traits were instrumental in our developmental success as a species.

Every one of us can be fooled, so don't cast any stones. Be skeptical of weird ideas, but go to the source and listen carefully and sympathetically. Approach it as a case study of self-deception. Try to pinpoint where logic went wrong. You might learn how to better avoid making mistakes yourself, and you just might have a few laughs in the process. Just remember, you're not laughing at an individual; you're appreciating the humor in the very human failings we all share.

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A PATENTLY FALSE PATENT MYTH—STILL!

from page 45

existed it could not have been seen "the other day" in 1915.

With the imaginative addition later of the names of Commissioners Ellsworth and Duell, the myth kept cropping up sporadically for decades but then received a major boost twenty years ago by the publication of the Morgan and Langford book in 1981 and the Cerf and Navasky book three years later. The 1985 TRW ad, which made use of the misinformation in these volumes, helped spread it more widely.

A particularly discouraging aspect of the repetition of this fable is that it is repeated by individuals who have research and fact-checking facilities at their disposal. For instance, in his book *The Road Ahead,* published in 1995, Bill Gates relates the myth as fact. He was chided for that in the February 1996 issue of *Scientific American* in these words: "If Bill Gates's grasp of the past is any guide, readers should take his visions of the future with a dose of skepticism." In view of the fact that eighty years earlier *The Scientific American* had made the same mistake it's encouraging to know that the editors felt compelled to correct Gates.

Two other prominent individuals who failed to take a second look are Carol Browner, former Administrator of the Environmental Protection Agency, and Hugh Downs, radio and television journalist. The former repeated the myth in an October 2000 talk to the National Press Club, and Downs stated in his July 17, 1996, "Perspective" radio commentary spiel that in "a handwritten note" Commissioner Duell had urged President McKinley to abolish the Patent Office because "Everything that can be invented has been invented." He was evidently so pleased with himself for being a purveyor of that news that he ended by telling his audience, "Remember that you heard it here first, on radio."

It would be the height of optimism to believe that efforts to debunk this myth will cause it to disappear. It's too good a story and lends itself too readily to those who are eager to make a point and to whom facts and truth are secondary. □

Human Nature Is ____ (Fill in the Blank)

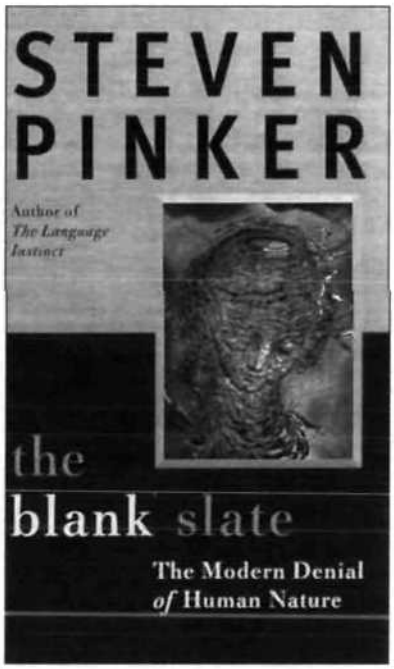
AUSTIN DACEY

The Blank Slate: The Modern Denial of Human Nature. Steven Pinker. Viking Penguin Press. New York. 0670031518. 528 pp. Hardcover, $27.95.

The next time you want to end a dinner conversation about some social problem or other, try using the argument that "it's human nature." If you are talking with intellectuals in the humanities or social sciences, chances are they will take you for a reactionary or an ignoramus, and excuse themselves. If you are talking with ordinary people, they probably will consider the matter settled by appeal to naturalness. The people to watch out for are the ones who respond by asking you what you mean by "human nature." The truth is, the concept is muddled.

For the ancient Greeks, a thing's nature had to do with those features that are possessed by all and only those things of its type—a nature is an *essence.* But essentialism about organisms has been undermined by Darwinism. Early modern thinkers like Hobbes, Locke, and Rousseau tried to discover "natural man" through a kind of thought experiment whereby one looks at contemporary people and subtracts from them those characteristics that appear to depend on society and politics. This was armchair anthropology.

Remarkably, empirical anthropologists throughout much of the twentieth century insisted that there really is no such thing as human nature. This they concluded from the remarkable diversity of human behavior cross-culturally. It does make sense to think that if a trait belongs to human nature, then it belongs to all individuals (or is at least "species-typical"—more on this later). However, it would be a mistake to seek universality at the level of behavior. In principle,



behavioral sameness can arise from shared circumstances as much as from shared "natures," and behavioral difference can arise from shared "natures" exposed to different circumstances.

Perhaps we need to look past variable behavior to an invariant structure underlying it. In the age of the Human Genome Project, it is tempting to equate this structure with DNA sequences. The difficulty with this suggestion is that we expect a concept of human nature to be useful in explaining things about us, yet genes themselves cannot explain anything. This is because organisms result from a process involving DNA interacting with its developmentally relevant environments—cellular, organismic, social, and ecological (not to mention stochastic developmental events). A revised version of the gene-centered view acknowledges the role of the developmental environments, but assigns analytical priority to genetic factors because they supply the information needed to build an organism, while non-genetic factors merely supply the raw construction materials. However, even if this description were accurate, there would be no rationale for assigning causal priority to informational factors as such.[1] It seems we cannot simply identify human nature with the genes.

Should we conclude that there is nothing in scientific or everyday knowledge that fills the bill? That would be a grave mistake, according to Steven Pinker, the MIT cognitive psychologist and popular author of *The Language Instinct* and *How The Mind Works.* In his ambitious new book, *The Blank Slate: The Modern Denial of*

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Human Nature, Pinker argues that the denial of human nature is no longer scientifically respectable. [An excerpt from the book appeared in the March/April 2003 SKEPTICAL INQUIRER.] He challenges head-on the radical environmentalist and social constructivist belief that our only significant biological endowment is an undifferentiated capacity for "learning" or "reasoning."

> sights and sounds. They must be equipped with mental machinery that can extract the beliefs and values underlying other people's behavior so that the children themselves can become competent members of the culture. (60)

Finally, Pinker deftly summarizes a diverse and fascinating assortment of evidence gathered from animal behavior, anthropology, archaeology, cognitive and developmental psychology, learnability theory, artificial intelligence, neuroscience, evolutionary biology, and genetics. He relies heavily on evolutionary psychology, the emerging research tradition that synthesizes evolutionary science and cognitive psychology. Evolutionary psychologists expect to find that the mind is mostly an array of complex, "domain-specific" information-processing mechanisms that on average contributed to reproductive success in ancestral environments.

One can think that there is a human nature—the Non-Blank Slate—without thinking that science now knows what it is—the Full Slate.

In the opening chapters of the book, Pinker brilliantly critiques this and related orthodoxies. First, he presents a series of broad, conceptual arguments informed by empirical research. Biology teaches us that wherever one finds complex adaptive systems in living nature, one finds evolution by natural selection behind them. Since the mind is surely a complex adaptive system, it stands to reason that it has been significantly shaped by selection. Meanwhile, common sense suggests that "there can be no learning without innate mechanisms to do the learning" (101). "Learning," "plasticity," and "culture" are not alternatives to biological or evolutionary explanations, but components of them:

> Culture . . . is not some miasma that seeps into people through their skin. Culture relies on neural circuitry that accomplishes the feat we call learning. Those circuits do not make us indiscriminate mimics but have to work in surprisingly subtle ways to make the transmission of culture possible. . . . for children to learn about culture they cannot be mere video cameras that passively record

What does this have to do with human nature? Many evolutionary psychologists think that cognitive adaptations (excepting sex differences) will be species-typical, or possessed by the average member of the species. Two leading researchers, Leda Cosmides and John Tooby, have dubbed this thesis the "psychic unity of humankind."[2] In it, we find a novel notion of human nature, distinct from eternal essences, pre-social states, common behaviors, or genes: our nature is our universal evolved psychology. In a number of places, Pinker endorses this concept of human nature:

> whatever the exact picture turns out to be, a universal complex nature will be part of it. I think we have reason to believe that the mind is equipped with a battery of emotions, drives, and faculties for reasoning and communicating, and that they have a common logic across cultures, are difficult to erase or redesign from scratch, were shaped by natural selection acting over the course of human evolution, and owe some of their basic design (and some of their variation) to information in the genome. (73)

Evolutionary psychologists look forward to the day (which may come) when they can compile a kind of cognitive *Gray's Anatomy*: a complete compendium of the mind, carved at the naturally selected joints. Of course, that day is nowhere near. Pinker is quick to admit that many important facts are not yet in, including "how our minds use the information coming in from the senses." He will defend a general model that can "embrace a variety of theories, present and future, and a range of foreseeable scientific discoveries" (73). Pinker's circumspection here is reasonable. After all, it is one thing to argue that the Blank Slate is prima facie implausible in light of a body of well-established evidence. It is another thing altogether to argue that the Blank Slate has been replaced by an alternative account. One can think that there is a human nature—the Non-Blank Slate—without thinking that science now knows what it is—the Full Slate.

Agnosticism about the Full Slate is all the more advisable at present because so much of the relevant science is immature. Consider evolutionary psychology. Despite some very promising studies, the field is rife with deep conceptual and methodological disagreements characteristic of a nascent discipline. For example, there is little agreement about what constitutes adequate confirmation of an adaptationist hypothesis that some cognitive trait evolved under natural selection. How does one rigorously define, in the

context of cognitive psychology, the "evidence of special design" that is the hallmark of selection? How does one bridge the evidentiary gap between showing, via an "optimality analysis," that *selection should have favored* a given cognitive trait under some possible ancestral conditions, and showing that those conditions actually obtained and *selection actually did favor the trait*?[3]

It is curious, therefore, that after making a powerful case for the Non-Blank Slate, Pinker devotes nearly half of the book to quite specific (and controversial) positive claims about altruism, morality, violence, gender roles, rape, art, and much else. In these sections, we are assured of the naturalness of numerous traits, including sexual orientation, women's desire to be with children, men's tolerance for danger and risk, the taste for risky sexual liaisons and infidelity, representational realism in art, and so on. This is a Full Slate, a substantive account of our nature that goes well beyond the basic supposition of a universal evolved psychology.

Pinker complicates matters by placing behavioral genetics among what he calls the sciences of human nature (135). Behavioral genetics studies twins, siblings, and adoptees to ascertain the heritability of personality traits such as openness to experience, conscientiousness, and "general intelligence." Overall, Pinker says, the field is revealing that "about half of the variation in intelligence, personality, and life outcomes is heritable—a correlate or an indirect product of genes" (374). He says that this "richer conception of human nature" will clarify controversies about childrearing (3), among other things. The problem is that behavioral genetics looks at the differences between individuals, not their commonalities. Therefore, it does not describe our "universal complex nature." Pinker also endorses psychologist Linda Mealey's view that psychopathy is actually not a pathology at all, but an adaptation, a strategy for defecting from reciprocal social interactions that evolved by frequency-dependent selection (in which the fitness of a behavioral strategy depends on the incidence of other strategies employed in the population). This would make *homo sapiens* what biologists call a polymorphous species—in which several "morphs" or "types" coexist in stable equilibrium. What happened to the psychic unity of humankind? Or would Pinker say that the Mealean psychopath is not a human being? When focusing on these issues, Pinker's usage of "human nature" tacitly shifts from "universal evolved psychology" to "whatever is due to genes."

After making a powerful case for the Non-Blank Slate, Pinker devotes nearly half of the book to quite specific (and controversial) positive claims about altruism, morality, violence, gender roles, rape, art, and much else.

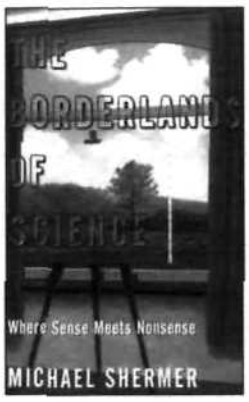
For all of its bold vision, rare insight, moral clarity, and polemical firepower, *The Blank Slate* spends precious little time analyzing just what it means to affirm or deny human nature. Half of the time, the book reads as an affirmation of the Non-Blank Slate. Here it is at its strongest, although it fudges the distinction between species-typical psychology and genetic factors as such. The other half of the time, it reads as an affirmation of the Full Slate, an advertisement for a soon-to-be-published *Gray's Anatomy* of the mind. Such a book truly would be before its time.

Notes

1. For over a decade, a new research program now known as "developmental systems theory" has attempted to reconceptualize the intractable nature-nature debate in terms of a "developmental system": the various genetic, cellular, organismic, and ecological structures interacting in a contingent ontogenetic history in which no single structure—including the DNA—is seen as "controlling" or "guiding" development. See Susan Oyama, *The Ontogeny of Information: Developmental Systems and Evolution*, 2nd ed. (Duke University Press, 2000); Paul E. Griffiths and Russell D. Gray, "Developmental Systems and Evolutionary Explanation," *Journal of Philosophy XCI*, 6 (1994): 277-304; Susan Oyama, Paul E. Griffiths and Russell D. Gray, eds., *Cycles of Contingency: Developmental Systems and Evolution* (Cambridge, Mass.: The MIT Press, 2003). In some important ways, Richard Dawkins' *The Extended Phenotype* (San Francisco: Freeman, 1982) accords with a developmental systems analysis.

2. John Tooby and Leda Cosmides, "The Psychological Foundations of Culture," in *The Adapted Mind: Evolutionary Psychology and the Generation of Culture*, Jerome H. Barkow, Leda Cosmides, and John Tooby, eds. (New York: Oxford University Press, 1992), 78–79.

3. For these and related issues, see Paul E. Griffiths, "From Adaptive Heuristic to Phylogenetic Perspective: Some Lessons from the Evolutionary Psychology of Emotion," in Harmon R. Holcomb, ed., *Conceptual Challenges in Evolutionary Psychology: Innovative Research Strategies* (Dordrecht: Kluwer Academic Publishing, 2001); "The Historical Turn in the Study of Adaptation," *British Journal for the Philosophy of Science* 47: 511–532. For a variety of general concerns about evolutionary psychology see Julie Fitness and Kim Sterelny, eds., *New Directions in Evolutionary Psychology* (Hove, Australia: Psychology Press, forthcoming); Harmon R. Holcomb, *Conceptual Challenges in Evolutionary Psychology: Sociobiology, Sex, and Science* (Albany, N.Y: SUNY Press, 1993); "Will Evolutionary Psychology Ever Become a Mature Science?" *Free Inquiry* 21: 51–54.



The Shaky Ground Between Science and Nonscience

PETER LAMAL

The Borderlands of Science: Where Sense Meets Nonsense. By Michael Shermer. Oxford University Press, New York, NY 10016, 2001. ISBN 0-19-514326-4. 360 pp. Hardcover, $27.50.

How do we draw the line between science and nonscience? Can we make this demarcation reliably? Are there cases where the difference between the two is fuzzy? The "Boundary Detection Problem," as Michael Shermer dubs it, is the focus of this book. Shermer considers and illustrates this foundational issue from three perspectives: theory, individuals, and history. In the introduction he points out that knowledge sets are fuzzy and the lines between them are blurry. Whether a particular claim belongs in the set labeled *science* or *pseudoscience* depends on both the claim and the definition of the sets. Shermer approaches the boundary problem by examining specific claims in some detail (e.g., remote viewing) in order to formulate principles for drawing the boundary. But he avers that many claims and theories are best thought of as existing in the borderland between science and nonscience.

Before outlining the ten questions that constitute his "Boundary Detection Kit," Shermer warns that an investigator should abide by *the fairness question:* "If I were to ask the holders of the claim if they feel that they and their beliefs were fairly treated, how would they respond?" This can alert investigators to the danger of setting up and then demolishing straw men. Shermer distinguishes normal science, nonscience, and borderlands science and lists examples of each, along with his judgment about their scientific validity. He says that because claims in each of the categories are fuzzy they can be reevaluated and moved to a different category as a result of changing evidence. All of the normal science claims (e.g., plate tectonics), he maintains, were once either in the nonscience or borderlands science categories, and it is the latter category to which the book is devoted.

In Part I, Shermer considers what he calls our "knowledge filter" that enables us to distinguish what is true from what is not. This section also includes consideration of borderland theories (e.g., athletic racial differences), at least some of which involve the hindsight bias and the confirmation bias as factors significantly contributing to the degree of plausibility they enjoy. Shermer discusses scientific paradigms and the controversy over punctuated equilibrium. It is here that Shermer presents his definition of science, one with which scientists would doubtless agree. He also offers definitions of *paradigm, paradigm shifts,* and *scientific progress* and uses punctuated equilibrium as the framework for discussing them.

Part II is concerned with individuals such as Copernicus, A.R. Wallace, Sigmund Freud, and Carl Sagan, all of whom were sometime dwellers in the borderland. Shermer points out that Copernicus's heliocentric system was, for scientific and ideological reasons, slow to be accepted. He describes why he labels Wallace "the heretic-personality," as well as Wallace's crossing the boundary from science to pseudoscience. Using the categories "hero as great man" versus "hero as myth," Shermer considers Freud and Darwin; I leave it to the reader to guess into which category he places each. Shermer then describes the balance that Sagan maintained between scientific orthodoxy and scientific heresy.

In Part III, Shermer explores how the study of humans' past can be accomplished using the scientific methods employed by such historical sciences as cosmology and paleontology. He also applies cognitive science to the study of historical figures and debunks the notion of the "miraculous" nature of genius. How science, especially revolutionary science, really works is illustrated by the priority dispute (Darwin or Wallace) over the theory of evolution. The Piltdown hoax is presented as a case study of a shift from science to pseudoscience, a warning that the data never just speak for themselves.

Although this book was an interesting and often informative read, it is not without problems. For example, Shermer cites tulipomania as a nineteenth century phenomenon. More importantly, we are presented with various kinds of data regarding the question of Carl Sagan's stature as a scientist, but perhaps the most important data, citations of his work by other scientists, is absent. In addition, Shermer reports the results of applying the Five Factor Personality Inventory of traits to Stephen Jay Gould and A.R. Wallace in order to assess such characteristics as their Conscientiousness and Openness to Experience. But the construct of "trait" is a contentious one among behavioral scientists. Even in the wake of disclaimers, it strongly suggests an innate determinant of individuals' behavior. Furthermore, "traits" are descriptions of certain of individuals' behavior patterns; they do not explain those behavior patterns. They are instead shorthand labels for the patterns. In his reliance on traits as explanatory, Shermer has, knowingly or not, moved into the borderland.

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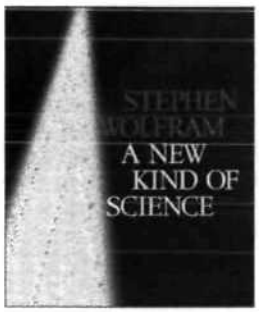

Divining Science's Simple Rules

ANUTOSH MOITRA

A New Kind of Science. By Stephen Wolfram. Wolfram Media, Inc., Champaign, IL, 2002. ISBN 1-57955-008-8. 1,197 pp. Hardcover, $44.95.

Patterns are curious things. They are ubiquitous, and discovering a few only whets the appetite for more, possibly leading to a sweet obsession to go quietly crazy. Stephen Wolfram is anything but crazy, but patterns have a curious hold on him. He is a well-known and respected scientist renowned as the originator of Mathematica, a software system for solving differential equations. He is also a proponent of Cellular Automata (CA), a computational scheme that in Wolfram's estimation would make differential equations obsolete as a tool for describing the workings of the physical universe. To support this ironic claim Wolfram has filled a book of over twelve hundred pages, *A New Kind of Science*, with exquisite patterns—of randomness, paradoxically. Wolfram's suspicions and certainties, strangely often synonymous in his writing, could have been succinctly laid out in a volume one-fifth the size of his much acclaimed and often maligned book. But the real questions to be asked are, Is it new? and Is it science? Claims of "new science" tend to raise skeptics' hackles, and rightly so. After all, claims of the supernatural and the paranormal often come clothed in precepts claiming to be beyond the capabilities of traditional science to understand them. They claim existence of phenomena that are only revealed when normal modes of perception are transcended. I do not believe Wolfram is making these claims, although his language of discourse is often reminiscent of them.

Wolfram talks about Cellular Automata and their ability to generate representations of complex phenomena. Cellular Automata may be thought of as synthetic universes. The behavior of the constituents of these universes is defined by simple rules as in a board game. Space is represented by a uniform grid with each cell containing a few bits of data. Time advances in discrete steps and at each step the data in each cell is updated based only on the states of the data in its closest neighboring cells. Wolfram has presented an abundance of examples of complex behavior arising out of CA governed by elementally simple rules. But is this new? Claims of new sciences appear every now and then. Two popular books with subtitles claiming the advent of new sciences have been published in the recent past (Bart Kosko's *Fuzzy Thinking: The New Science of Fuzzy Logic* and James Gleick's *Chaos: Making A New Science*). Both of them have disappeared from popular imagination without kindling a revolution. Toffoli and Margolus, in their book with a far more appropriate subtitle, *Cellular Automata Machines: A New Environment for Modeling*, tell us that CA have been invented many times under different names, and somewhat different concepts have been circulated under the same name. CA were introduced in the late 1940s by John Von Neumann. I have followed Wolfram's work with great interest, CA modeling of fluid mechanics in particular, over a couple of decades and would hate to see the interesting parts of his work marginalized because of inflated claims of newness.

In a nutshell Wolfram's claim appears to consist of the following line of argument:

- Algorithms based on simple rules can generate complex behavior of systems.
- The universe is a complex system.
- Therefore the universe operates through simple rules.

The obvious flaws of this syllogism notwithstanding, the analyses given in the book warrant serious critical evaluation. The usefulness of CA in modeling the behavior of complex systems has been well established in many fields of science including fluid mechanics—a field Wolfram draws upon often to illustrate his arguments. What must be noted, however, is that his claims go far beyond just modeling. He finds no reason not to believe that the universe is in fact a giant cellular automaton. A skeptic will immediately notice the multiple negation. I suppose Wolfram is open to being proven wrong, but he places the burden of proof on everybody else, shrugging off the responsibility to furnish extraordinary proof for his own extraordinary claim. This confusion between models and actual physical systems—the confounding of the metaphor and the real thing—pervades the whole of Wolfram's book. Skeptics are well aware that indulging this synonymy between symptom and cause can easily lead one to accept fallacious arguments such as "after this, therefore because of this."

Models have always been used in scientific simulation of physical phenomena. Models are approximations used to describe the aggregate behavior of physical systems, insofar as these behaviors are known, without actually reproducing the workings of the system in its minutest details. Take the example of fluid turbulence, Wolfram's favorite topic. Theoretical models are essential to studying turbulence because the exact mechanism through which turbulence is generated is not yet known to science. Traditionally, turbulence theories attempt to model fluid turbulence phenomena through statistical averages of the velocity field. This field is, however,

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itself an average over microscopic molecular motion. I was first drawn to Wolfram's work by his depiction of flow-fields showing structures characteristics of turbulent flow that he had generated by CA without incorporating any physical principles whatsoever.

However, these structures were visible only when the states of the individual cells in the grid were averaged over large numbers of cells. In this sense of predicting averaged behavior, therefore, CA fares not much better than traditional models of turbulence. Claiming a new science that makes traditional science obsolete is thus unwarranted; it provides just another model for studying turbulence. There are many precedents in physics of simple discrete models able to provide correct approximations of complexities of real world phenomena. This in no way implies that these models actually generate the behavior of the system at its microscopic level.

Philosophically the real question here must be considered in both its epistemological and ontological aspects. In the context of modeling it is sufficient to know the rules that effectively describe the aggregate behavior of a system. These models can then be utilized and refined to gain further insight into and knowledge about the system. But what does this knowledge consist of? In scientific inquiry one seeks to know what actually happens, not just what the end result looks like. Here Wolfram presents us with a curious argument. He informs us that CA's ability to actually reproduce physical phenomena entirely down to the microscopic interactions can only be realized when viewed from an as-yet indefinable higher form of perception and analysis. This leads him to make dubious statements such as the following: the very fact that we have so far failed to discover any evidence for extraterrestrial intelligence may itself conceivably already be a sign that higher forms of perception and analysis may be in use. This is not new either. Skeptics have heard this line of argument before from proponents of ESP.

Let me now explain why despite all this I consider Wolfram's book an important one. Science is not best served by models which slavishly obey all of our wishes by faithfully reproducing what we have programmed into them. Science needs models that give back more than what we have put into them. A good way to start may be to put in as little as possible, to program into our models the simplest possible rules. Wolfram shows that simple rules can generate most classes of complex behavior and the simplest of these rules can generate all the other rules. Validation of this principle, termed "Principle of Computational Equivalence" by Wolfram, will be a significant step in the journey toward the holy grail of science—the definition of the simple rules that describe the universe. Wolfram has not accomplished this validation, but one can hope.

A Tour of the Halls of Flummery

BENJAMIN RADFORD

The Museum of Hoaxes: A Collection of Pranks, Stunts, Deceptions, and Other Wonderful Stories Contrived for the Public from the Middle Ages to the New Millennium. By Alex Boese. Penguin Putnam, New York. 2002. ISBN 0-525-94678-0. 266 pp. Hardcover, $19.95.

Hoaxes and the history of hoaxing are important elements in understanding practical skepticism. The very notion that perhaps a remarkable story or account may not be entirely (or at all) true is, at its heart, a skeptical one. In a way, the history of the paranormal closely parallels the history of hoaxes. From Bigfoot to Spiritualist mediums, from the Turin Shroud to the Roswell crash, there is hardly a category of the unexplained or paranormal that does not have a robust history of fraud and hoaxing. Alex Boese, a graduate student in San Diego and curator of the Museum of Hoaxes Web site, guides readers through his virtual museum in his new book *The Museum of Hoaxes.*

Some of the hoaxes will be familiar to longtime SKEPTICAL INQUIRER readers, such as the Cardiff Giant (said to be a petrified giant as described in the Bible), the Cottingley Fairies, and crop circles. The book, however, also includes a fascinating survey of lesser-known hoaxes. Boese covers hoaxes perpetrated by respected luminaries like Benjamin Franklin and Mark Twain as well as inveterate pranksters such as Joey Skaggs. The book's presentation of the early history of hoaxes is very good and enlightening. Sir John Mandeville, Marco Polo, and other early explorers were accused—with good reason—of faking some of their seminal travel observations and journals.

The Museum of Hoaxes begins with a good (if arguable) discussion delineating differences between hoaxes, pranks, urban legends, frauds, and tall tales. There's some overlap, but he settles on the careful definition of *hoax* as "a deliberately deceptive act that has succeeded in capturing the attention of the public."

Boese provides an insightful discussion of the public's role in hoaxing. Hoaxes do not occur in a vacuum; the public's reaction to the hoax is an important element of the hoax. With an historian's perspective, Boese points out that current notions of truth are quite different from medieval ones. These days, we have a modern sophistication (and desire to discern truth) that might have seemed

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bizarre to our ancestors. Truth, after all, is the basis by which a hoax, tall tale, or other deception is measured. A mutable (or postmodern) truth leaves little basis upon which to discern fact from fraud. "The medieval world treated the concept of truth allegorically and spiritually, while we treat it scientifically. Claims that we would regard as obviously false were regarded by the medieval mind as true if they revealed a deeper underlying metaphysical meaning about the world. . . . Public life was highly localized and fragmented. Sharing of information between communities was sporadic and unreliable. As a result secrecy, not openness, became the fountain of medieval knowledge."

There are many books on hoaxes. Some, like Gordon Stein's *Encyclopedia of Hoaxes* and Carl Sifakis's *Hoaxes and Scams*, are excellent encyclopedic resources. Others, such as *Outrage: The Story Behind the Tawana Brawley Hoax*, are exhaustive, detailed analyses of a particular hoax. But among this lot, there is usually little discussion of the *nature* of hoaxes. *The Museum of Hoaxes* admirably fills the gap between the exhaustive catalogues of hoaxes and single-case studies. The book does a fine job of serving as a reference source; it not only has an index but also lists hoaxes by category and includes an extensive chapter-by-chapter list of references for further reading.

There are some apparent omissions. In the section on literary hoaxes, there's no mention of *The Amityville Horror*, or Mark Helfrich's 2000 book *Naked Pictures of My Ex-Girlfriends* (though the story of Sony's fake film reviewer David Manning is briefly recounted). There's also no mention of terrorism hoaxes, such as the literally thousands of hoaxes that followed the September 11, 2001, attacks and the anthrax-laced letters to politicians (though perhaps the book's deadline was too early to include a discussion of these).

I was often left wanting more information on a given topic or hoax, and it was at times frustrating to read an entry of only a few paragraphs. Still, a book the size and scope of *The Museum of Hoaxes* (like any museum) cannot hope to be complete and authoritative; new hoaxes occur all the time. The best a book like this can do is to bring readers a general survey of hoaxes, their nature and perpetrators, and offer some tips on how to avoid being hoaxed. In that, *The Museum of Hoaxes* is a valuable skeptical book for both serious research and casual amusement.

'Einstein' Exhibit at the American Museum of Natural History

JERRY KURLANDSKI

If Einstein hadn't been born, would we have had to create him? That's one of the big unasked questions lurking behind the Einstein exhibition now showing at the American Natural History Museum in New York City. A special program of events, talks, performances, and workshops both directly and tangentially related to Einstein's life and work runs concurrently.

The exhibition proper—sans events, talks, and whatnot—serves as a mixed-media biography of the scientist. We learn the kind of summary information that can be picked up from an article in, for example, *The World Book Encyclopedia*. What makes the tour worthwhile is its artful marriage of encyclopedia-like text with the traditional strengths of a museum—photographs, manuscript reproductions, and interactive presentations. The curator, Michael M. Shara, has done a fine job; still, there is little in his show that is going to raise any eyebrows.

The same cannot be said of the special program. While many of the talks have unsurprising themes ("Einstein's Relativity: Past, Present and Future," "Einstein: Patents and Inventions"), other have more intriguing titles ("The Soul of Einstein: A Dialogue on the Moral, Religious, and Philosophical Life of Albert Einstein"). The events billed as "performances" explore the Einstein theme through such means as cabaret, musical narration, and juggling.

Perhaps one day the distinction between the modern-day museum exhibition and its symbiotic gift shop will cease to exist. In the meantime, a pair of heavy glass doors separate this particular exhibition from its fund-raising counterpart. Through those doors you can purchase, of course, Einstein T-shirts and mugs, as well as Einstein Beanie babies, plates, and refrigerator magnets.

Taken as a whole, the exhibition admirably fulfills its duty to entertain, educate, and even inspire. What it lacks, however, is any hint of self-examination. There is much on Einstein the genius,

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but no mention of Einstein the pop icon, though the latter would seem to have as large a part in the exhibition as the former. In his essay "Einstein's Brain," written around the time of the scientist's death in 1955, Roland Barthes pointed out the discord between photographs and cartoons of Einstein. Typically, photographs showed him before a blackboard covered with complex mathematical equations, whereas cartoons depicted the theoretical physicist "having just written on an empty blackboard, as if without preparation, the magic formula" $E=mc^2$. From Barthes's observation we can safely conclude that, even while he was still alive, Einstein the genius had to share the limelight with Einstein the pop star. What Einstein the human being thought about all this is not discussed in the exhibition and its programs.

Which brings us to the question asked at the beginning of this review. If Einstein hadn't been born, would some other scientist have been elevated to the same position? And if so, who would have been thrust into this curious, amalgamated role of scientist, sage, and saint? Darwin, perhaps? I am inclined to believe that the post could only be filled by an American still alive after World War II, when advanced methods of mass communication came into existence. Perhaps, then, the answer to the question is No—no other scientist would have enjoyed and suffered the same fame as Einstein.

In any case, after enjoying and appreciating this exhibition, one nevertheless regrets that the question—or others like it—is not even raised. □

SCIENCE BEST SELLERS

Top Ten Best Sellers in **New York**

1. ***The Golden Ratio: The Story of Phi, the World's Most Astonishing Number***
   Mario Livio
   Broadway Books

2. ***Fast Food Nation: The Dark Side of the All-American Meal***
   Eric Schlosser
   Houghton Mifflin

3. ***Guns, Germs, and Steel: The Fates of Human Societies***
   Jared Diamond
   W.W. Norton and Co.

4. ***The Art of War***
   Samuel B. Griffith/Sun Tzu
   Oxford University Press

5. ***Sync: The Emerging Science of Spontaneous Order***
   Steven Strogatz
   Hyperion Press

6. ***Salt: A World History***
   Mark Kurlansky
   Penguin

7. ***Merriam-Webster's Collegiate Dictionary***
   Merriam-Webster, Inc.

8. ***The Life and Death of Planet Earth: How the New Science of Astrobiology Charts the Ultimate Fate of Our World***
   Peter Ward, Dan Brownlee
   Times Books

9. ***The Feeling Good Handbook***
   David D. Burns
   Plume

10. ***Looking for Spinoza: Joy, Sorrow and the Feeling Brain***
    Antonio Darnasio
    Harcourt

By arrangement with Amazon.com, March 2003.

Listing does not preclude future review.

The Inner Eye: Social Intelligence in Evolution. Nicholas Humphrey. Oxford University Press, 198 Madison Ave., New York, NY 10016, 2002. 188 pp. $12.95, softcover. Where does consciousness come from? What is it? Where is it taking us? The theoretical psychologist Nicholas Humphrey presents a short work growing out of a mid-1980s series of U.K. television programs on the evolution of human consciousness. Includes a discussion of the evolution of intelligence and its relation to social life and his theory about the development and uses of human consciousness.

The Mind Made Flesh: Frontiers of Psychology and Evolution. Nicholas Humphrey. Oxford University Press, 198 Madison Ave., New York, NY 10016, 2002. 366 pp. $18.95, softcover. A new collection of essays, lectures, and articles dealing with the uneasy relation between minds and bodies. The focus is centered on the author's longstanding interest in evolutionary psychology, which he feels has emerged as "the most fertile field of all psychology." Perennial issues of consciousness, justice, social understanding, and spirituality are dealt with. Throughout, Humphreys takes issue with received ideas.

Science Literacy for the Twenty-First Century. Edited by Stephanie Pace Marshall, Judith A. Scheppler, and Michael J. Palmisano. Prometheus Books, 59 John Glenn Drive, Amherst, NY 14228-2197, 2003. 321 pp. $29, hardcover. A collection of essays by eminent scientists and science educators, including Bruce Alberts, Howard Gardner, Margaret Geller, Stephen Jay Gould, George Keyworth, Lawrence Krauss, Leon Lederman, Sheila Tobias, and James Trefil. They all involve suggesrions for improving the standards of scientific literacy throughout the world and science education in the United States. The essays were invited in honor and celebration of Nobel laureate physicist Leon Lederman, who has devoted much of his life to these concerns.

The Science Wars: Debating Scientific Knowledge and Technology. Edited by Keith Parsons. Prometheus Books, 59 John Glenn Drive, Amherst, NY 14288, 2003. 325 pp. $21, paper. From creationists on the right to postmodernists on the left, science as a way of knowing has been subjected to intense critical scrutiny. Editor Parsons, a philosopher (University of Houston) and author, offers the best arguments the science critics have presented and some of the spirited defenses of science in response. Other essays go deeper to analyze the underlying issues. The book is organized into four parts: The Constructivist Challenge, Feminists on Science, Science and Postmodernism, and Conservative Critiques of Science. Scholars whose writings are represented range from Bruno Latour, Sandra Harding, and Phillip Johnson on the critics' side to Paul R. Gross, Normal Levitt, Steven Weinberg, and Robert T. Pennock on behalf of science.

Spin Doctors: The Chiropractic Industry Under Examination. Paul Benedetti and Wayne MacPhail. With a foreword by Dr. Brad Stewart and an afterword by Dr. Stephen Barrett. The Dundurn Group, 8 Market St., Suite 200, Toronto, ON M5E 1M6, 2003. 296 pp. $24.99, softcover. Two Canadian investigative journalists examine chiropractic. They explore its roots, its treatment methods, the misleading information disseminated about chiropractic care, and the taxpayer money used to pay for treatments. They also explain how to protect yourself from dangerous adjustments and bogus treatments.

Van Nostrand's Concise Encyclopedia of Science. Edited by Christopher G. DePree and Alan Axelrod. Wiley Publishers, 605 Third Avenue, New York, NY 10158, 2003. 820 pp. $40, hardcover. Designed for "anyone who needs speedy access to authoritative information on a large number of scientific disciplines," this encyclopedia claims to cover every key concept, theory, and area of research in science. Though well-written, many of the 5,000-plus entries are of dubious scientific importance—why is the quetzal bird ("the best known of all trogons") included, and why is half a page of space spent illustrating it? Useful and accessible but with spotty organization.

What Your Astronomy Textbook Won't Tell You: Clear, Savvy Insights for Mastery. Norman Sperling. Foreword by David Levy. Everything in the Universe, 413 Poinsettia Avenue, San Mateo, CA 94403 (e-mail: wonttell@en.com), 2002. 183 pp. $23.70, softcover. An interesting approach to teaching good astronomy by discussing things that won't be discussed in formal textbooks: misconceptions, blunders, confusing jargon, how bogus claims are debunked, things we don't yet know, and so on. All are short and lively.

—Kendrick Frazier and Benjamin Radford

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How *Not* To Review Mediumship Research

GARY E. SCHWARTZ

Most rational scientists agree that the credibility and integrity of a review of a body of research is that it includes all the important information, not just the reviewer's favored information. Ray Hyman's review "How *Not* To Test Mediums" (January/February 2002) is a textbook example of the selective ignoring or dismissing of historical, procedural, and empirical facts to fits one's preferred interpretation. The result is an inaccurate, mistaken, and biased set of conclusions of the current data.

Hyman is a distinguished professor emeritus from the Department of Psychology at the University of Oregon, who has had a longstanding career as a skeptic focused on uncovering potential flaws in parapsychology research. Hyman is well skilled in carefully going through the conventional checklist of potential sources of experimental errors and limitations in research designs.

Hyman's overall appraisal of the research conducted to date is implied by his conclusion: "Probably no other extended program in psychical research deviates so much from accepted norms of scientific methodology as does this one."

Is Hyman's summary conclusion based upon a thorough review of the total body of research? Or does it reflect the systematic ignoring of important historical, procedural, and empirical facts—a cognitive bias used by the reviewer in order to maintain his belief that the phenomenon in question is impossible? As I document below, Hyman resorts to (consciously and / or unconsciously) selectively ignoring important information that is inconsistent with his personal beliefs.

Selective ignoring of facts is not acceptable in science. It reflects a bias that obviates the purpose of research and disallows new discoveries. I have made the statement that the survival consciousness hypothesis does account for the totality of the research data to date. Of course, this does not make the survival hypothesis the only or correct hypothesis—my statement reflects the status of the evidence to date, not necessarily the truth about the underlying process. This is why more research is needed.

Note that I do not use the word "believe" in relationship to the statement. This is not a belief. It is an empirical observation derived from experiments.

It is correct that some of the single-blind and double-blind studies have weaknesses—we discuss the experimental limitations at some length in our published papers as well as in *The Afterlife Experiments.* However, these weaknesses do not justify dismissing the totality of the data as mistaken or meaningless. Quite the contrary, an honest and accurate analysis reveals that the data, in total, deserve serious consideration.

Our research presents *all* the findings—the hits and the misses, the creative aspects of the designs and their limitations—so that the reader can make an accurate and informed decision. What we strive for is seeking the truth as reflected in Harvard's motto "Veritas."

I appreciate Hyman's effort to outline some of the possible errors and limitations in the mediumship experiments discussed in *The Afterlife Experiments.* However, as Hyman emphasizes in his review, I do "strongly disagree" with him about his interpretations. The two fundamental disagreements I have with Hyman's arguments are:

1. Hyman has chosen to ignore numerous historical, procedural, and empirical facts that are inconsistent with his interpretive descriptions of our experiments; and
2. Hyman has chosen not to acknowledge the totality of the findings following Occam's heuristic principle as a means of integrating the total set of findings collected to date.

Space precludes my providing a detailed and thorough commentary here illustrating how pervasively Hyman ignores and omits important information. (An extensive commentary has been published on various Web sites, including www.openmindsciences.com.) Four samples of important ignored facts are provided below.

Selective Ignoring of Historical, Procedural, and Empirical Facts

Veritas 1: In his review, Hyman failed to mention the important historical fact that our mediumship research actually began with double-blind experimental designs. For example, the published experiment

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referred to in *The Afterlife Experiments* as "From Here To There and Back Again" with Susy Smith and Laurie Campbell was completed almost a year before we conducted the more naturalistic multi-medium/multi-sitter experiments involving John Edward, Suzanne Northrop, George Anderson, Anne Gehman, and Laurie Campbell. The early Smith-Campbell double-blind studies did not suffer from possible subtle visual or auditory sensory leakage or rater bias—and strong positive findings were obtained.

Our decision to subsequently conduct more naturalistic designs (which are inherently less controlled), was made partially for practical reasons (e.g., developing professional trust with highly visible mediums) and partly for scientific ones (e.g., we wished to examine under laboratory conditions how mediumship is often conducted in the field).

Conclusion: Hyman makes a factually erroneous criticism when he reports that double-blind experiments were initiated only late in our research program, and therefore makes a serious interpretative mistake when he decides that all the early data can be dismissed because they were not conducted double-blind.

Veritas 2: In an exploratory double-blind long distance mediumship experiment where George Dalzell (GD) was one of six sitters and Laurie Campbell (LC) was the medium, Hyman states "because nothing significant was found, the results do not warrant claiming a successful replication of previous findings."

However, Hyman minimizes the fact that the number of subjects in this exploratory experiment was small (n=6). More importantly, Hyman fails to cite a important conclusion that we reached in the discussion: "If the binary 66 percent figure approximates (1) LC's actual ability to conduct double-blind readings, coupled with (2) the six sitter's ability, on the average, to score transcripts double-blind, the 66 percent figure would require only an n of 25 sitters to reach statistical significance (e.g., $p < .01$)."

Hyman fails to mention that NIH, for example, requires that investigators who apply for research grants calculate statistical power and sample size to determine what n is required to obtain a statistically significant result. This is accepted scientific practice and is required for obtaining NIH funding.

Just as I don't take the claims of the mediums on faith, I don't take the claims of the magicians on faith either.

Conclusion: Hyman would rather dismiss the fact that the highly accurate ratings obtained in the single-blind published study for GD were indeed replicated in the double-blind published study, than to admit the possibility that individual differences in sitter characteristics are an important and genuine factor in mediumship research.

Veritas 3: It is curious that among the many examples of readings provided in *The Afterlife Experiments*, one early subset (cluster/pattern) of facts happened to fit Hyman nicely. It is true that mention of the "Big H," a "father-like figure," an "HN sound" would fit Hyman's father like it did the sitter's husband mentioned in the book.

Hyman chose not to report the fact that many other pieces of specific information also reported for the "Big H" did *not* fit Hyman but did fit the sitter precisely. Moreover, Hyman consistently failed to report scores of examples from readings reported verbatim in the book that were highly unusual and unique to individual sitters (e.g., John Edward seeing a deceased grandmother having two large poodles, a black one and a white one, and the white one "tore up the house").

Conclusion: The reason Hyman failed to mention these numerous examples is because they contradict the conclusion Hyman chose to accept—that the information, by chance, could fit multiple sitters—an erroneous conclusion that can be reached only if we do what Hyman did and accept the information selectively.

Veritas 4: Hyman's conclusion that experienced cold readers can readily replicate the kinds of specific information obtained under the conditions of our experiments is mistaken at best and deceptive at worst.

Under experimental conditions where (a) professional cold readers do not know the identity of the sitters (i.e., cheating is ruled out), and (b) cold readers are not allowed to see or speak with the sitters (i.e., cueing and feedback is ruled out), it is (c) impossible for cold readers to use whatever pre-obtained sitter specific information they might have obtained, and (d) impossible for cold readers to use their feedback tricks to help them get information from the sitters.

At the two-day meeting I convened in Los Angeles of seven highly experienced professional mentalist magicians and cold readers, they all agreed that they could not apply their conventional mentalist tricks under these strict experimental conditions. However, a vocal subset (Hyman was one of the three), made the unsubstantiated claim that if they had a year or two to practice, they might be able to figure out a way how to fake what the mediums were doing.

My response to this vocal subset was simple. It was "show me." Just as I don't take the claims of the mediums on faith, I don't take the claims of the magicians on faith either. I am a researcher. Mentalist magicians who make these claims will have to "sit in the research chair" and show us that they can do what they claim they can do.

Thus far, the few cold readers who

have made these claims have refused to be experimentally tested. They have been unwilling to demonstrate in the laboratory that they can't do what the mediums do under these experimental conditions; and they have been unwilling to demonstrate at a later date that their performance can improve substantially with practice.

Conclusion: The claim that cold reading can account for the research findings is not supported when the experimental procedures are honestly taken into account.

Failure to Integrate Information and Appreciate the Process of Discovery

In most areas of science, no single experiment is perfect or complete. Different experiments address different conditions and different alternative explanations to different degrees. The challenge is to connect the dots of the available data and integrate the complex set of findings using the fewest number of explanations (i.e., Occam's razor).

Hyman reveals in his review that he learned as a teenager that it was easy for him to fool many people with palm reading. It is also quite easy to fool many people with fake mediumship, as anyone trained in cold reading will tell you. I have studied a number of books on cold reading and have taken some classes on cold reading myself. However, just because it is possible sometimes to be fooled (especially by the masters of magic) doesn't mean that everyone is fooling you.

Hyman reluctantly agrees that it is improbable that the totality of our findings can be explained by fraud. As a result, his preference is to propose that the set of findings collected to date must involve a complex set of subtle cues providing information in some studies, cold reading techniques being used in some studies, rater bias providing inflated scores in some studies, and chance findings in some studies. The idea that mediums might be obtaining anomalous information that can most simply and parsimoniously be explained in terms of the continuance of consciousness is presumed categorically to be false by Hyman until proven otherwise.

To me the question of whether or not mediums are obtaining anomalous information is a purely scientific one, to be revealed through a program of systematic research.

I make no such categorical assumptions, one way or the other. To me the question of whether or not mediums are obtaining anomalous information is a purely scientific one, to be revealed through a program of systematic research. Such research must be conducted by multiple laboratories. The reason for publishing findings, as they emerge, is to encourage other investigators to conduct their own experiments, and then integrate the totality of the findings.

However, the truth is, it is impossible to integrate the totality of the findings in any area of science if one selectively (consciously or unconsciously) ignores those specific findings that do not fit one's preferences or biases.

Scientific Integrity and Changing One's Beliefs

I admit, quite adamantly, that I do have one fundamental bias—my bias is to use the scientific method to discover the truth, whatever it is. Discovering the truth cannot be achieved through selective reporting of history, procedures, and data.

So what is the truth at the present time, based upon the available data? When the totality of the history, procedures, and findings to date are examined honestly and comprehensively—not selectively sampled to fit one's particular theoretical bias—something anomalous appears to be occurring in the mediumship research, at least with a select group of evidence-based mediums.

Over and over, from experiment to experiment, findings have been observed that deserve the term *extraordinary*. In our latest double-blind, multi-center experiments, stable individual differences in sitters have been observed that replicate across laboratories and experiments. The observations are not going away—even with multi-center, double-blind testing.

Hyman once told me, "I have no control over my beliefs." When I asked him what he would conclude if a perfect large sample multi-center double-blind experiment was conducted, his response was, "I would want to see your major multi-center, double-blind experiment replicated a few times by other centers before drawing any conclusions."

This conversation is revealing psychologically. Until multiple perfect experiments are performed and published, Hyman would rather believe that the totality of the findings must be due to some combination of fraud, cold reading, rater bias, experimenter error, or chance—even if this requires that he selectively ignores important aspects of the history, designs, and findings in order to hold on to his belief that he (or we) are being "fooled."

Why spend the time and money conducting multiple multi-center, double-blind experiments unless there are sufficient theoretical, experimental, and social reasons for doing so?

The critical question is, "Is it possible that consistent with the actual totality of the data collected to date—viewed historically (e.g., the observations of William James) as well as across disciplines (e.g., from anthropology to astrophysics)—that future research may lead us to come to the conclusion that consciousness is intimately related to energy and information,

and that consciousness, as an expression of dynamically patterned energy and information, persists in space like the light from distant stars?"

This is ultimately an empirical question; it will be answered by data, one way or the other. If positive data are obtained—and I emphasize if—accepting the data will require that we be able to change our beliefs as a function of what the data reveal. *The Afterlife Experiments* was written to encourage people to keep an open mind about what the future research may reveal.

Epilogue: What is a Magazine's Responsibility?

If the SKEPTICAL INQUIRER wishes to be viewed as being a credible publication, more like the *Philadelphia Inquirer* than the *National Enquirer*, it should take responsibility for fact checking its articles and correcting mistakes caused by simple errors and/or the selective ignoring of important information.

For example, Hyman's review begins by stating that I was a professor at Yale University for twenty-eight years—the fact is, I was at Yale for twelve years. If the SKEPTICAL INQUIRER had not chosen to keep Hyman's review secret, and had asked me to fact check Hyman's review, I would have gladly done so, and therefore enabled both the magazine and the reviewer to correct at least the obvious errors of fact. Clearly, little mistakes, compounded by big mistakes, do not make for a credible publication or review.

I am taking a strong position about accuracy of reporting here not because of the ultimate validity of the survival hypothesis (i.e., whether it is true or not, since that is an experimental question) but because of the nature of scientific reviewing process itself.

The selective ignoring and omission of important information cannot be condoned in either reviewing or publishing. It must be exposed and understood, regardless of the specific research area that is being reviewed or the specific person doing the reviewing.

Note that my argument is not with Hyman as a person, nor with the SKEPTICAL INQUIRER as a publication. My concern is about the process by which Hyman has written his review, and the responsibility of SKEPTICAL INQUIRER to decrease the likelihood that this kind of mistaken review will be published in the future. There is a bigger lesson here. It is worth considering, and correcting.

Acknowledgments

I thank a number of my colleagues who have graciously taken the time to provide me with useful feedback about this commentary. They include Peter Hayes, Ph.D., Katherine Creath, Ph.D., Stephen Grenard, Ph.D., Donald Watson, M.D., Emily Kelly, Ph.D., Lonnie Nelson, M.A., and Montague Keen. The comments provided here are those of the author, not necessarily those of my colleagues.

Hyman's Reply to Schwartz's 'How *Not* To Review Mediumship Research'

RAY HYMAN

I cannot, of course, respond in detail within the allotted space to each of Schwartz's arguments. Instead, I will comment on his major points and conclude with a general reaction to his rebuttal.

1. "Hyman resorts to . . . selectively ignoring important information that is inconsistent with his personal beliefs."

In preparing my critique of his research program, I not only read *The Afterlife Experiments* carefully, I also scrutinized in detail every report of his research that was available. It was not possible to discuss each separate piece of information in my critique. I took each item into account, however, in making my assessment of the research. I chose to focus my discussions on those items that Schwartz and his colleagues had emphasized as the strongest outcomes amongst their findings. I have refereed and reviewed research reports for more than fifty years for many of the major scientific publications and for major granting agencies. I applied the same standards to my evaluation of the afterlife experiments that I have used in my other assessments.

2. ". . . Hyman failed to mention the important historical fact that our mediumship research actually began with double-blind experimental designs."

As his example he refers to his experiment with the mediums Susy Smith and Laurie Campbell that "was completed almost a year before we conducted the more naturalistic multi-medium/multi-sitter experiments involving John Edward, Suzanne Northrop, George Anderson, Anne Gehman, and Laurie Campbell. The early Smith-Campbell double-blind studies did not suffer from possible subtle visual or auditory sensory leakage or rater bias—and strong positive findings were obtained."

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This is a peculiar example to use as a model of a controlled, double-blind experiment. The experiment involved having Susy Smith, designated as Medium One, apparently contact four deceased persons: her own mother, William James, Linda Russek's father, and Schwartz's father. Smith made a drawing for each of these departed individuals supposedly with their input. She also made a "control" drawing. Laurie Campbell, designated as Medium Two, was then requested to independently attempt to contact these departed individuals and, using the information obtained from them, to try to match each drawing to the associated departed individual. Campbell attempted to contact the departed entities during two sessions in the presence of three experimenters. Campbell is described as being "blind" to personalities of the four departed individuals. However Schwartz, who was not blind to the personalities of these entities, was not only present during these sessions but actively trying to convey this information (through "telepathy") to Campbell. This unnecessary blunder compromises whatever blinding would have existed between Medium Two and the personalities of the departed individuals. No psychic investigator would be surprised if Laurie Campbell came up with some correct information such as the gender and other descriptors of the departed individuals under these conditions.

Another defect of this phase of the experiment is that no provisions were made to use a systematic and objective method for assessing the accuracy of Medium Two's descriptions. The evaluation of the information for this stage of the experiment was subjective.

During the sittings with Medium Two, all the experimenters were blind as to which drawing was associated with which departed individual. (Although it is plausible that one might be able to make some reasonable guesses, given the characters of each of the departed individuals, which type of drawing would go with each one.) Unfortunately, the experimenters then make another serious, and completely unnecessary, blunder when it came time to see if Medium Two could accurately match the drawings with the appropriate individual. The experimenters brought Medium Two and Medium One together. Medium One then displayed the drawings she had made to represent each individual. Medium Two then attempted to match the drawings to the appropriate sources in the presence of Medium One. Ironically, the experimenters openly admit that this could allow clues about the correct matching through the "Clever Hans" phenomenon. They dismiss this as possibility because Campbell was able to correctly match only one of the five drawings to its appropriate source.

At this point in the experiment the report becomes especially murky. Presumably, the experiment has failed. However, the experimenters inexplicably have Medium Two try *again* to match the drawings to their appropriate source. This second attempt is made after she is shown an explicit summary of her comments about the pictures and the departed individuals. Campbell correctly matches the five drawings (including the control) in this second attempt. No reason is given for giving the medium two tries at matching the drawings, nor do the experimenters tell us how they justify asking the medium to redo her matching. Probably these and other questionable aspects of the procedure are moot given that the possibility of blinding was compromised.

Schwartz has really not answered my criticisms. A close reading reveals that he does not deny the various failings I have divulged in his research.

Schwartz and his colleagues, in their published paper, describe this as an "exploratory study." The proceedings seem to have been improvised at each stage. Certainly, no competent investigator would plan to unnecessarily compromise experimental blinding at the two most critical points of the data collection. Nor does it make sense to design an experiment wherein the medium is given two chances at getting the matching correct. I simply was applying the principle of charity in not discussing this botched experiment.

3. "In an exploratory double-blind long-distance mediumship experiment . . . Hyman states 'because nothing significant was found, the results do not warrant claiming a successful replication of previous findings.' However, Hyman minimizes the fact that the number of subjects in this exploratory experiment was small (n=6). More importantly, Hyman fails to cite a(n) important conclusion that we reached in the discussion: If the binary 66 percent figure approximates (1) LC's actual ability to conduct double-blind readings, coupled with (2) the six sitters' ability, on the average, to score transcripts double-blind, the 66 percent figure would require only an n of 25 sitters to reach statistical significance (e.g. < .01)."

This part of Schwartz's rebuttal, like all the other parts, strikes me as both bizarre and off the mark. First, we need to clear up some mistakes and/or misunderstandings. Schwartz confuses the sample statistic with the population (or hypothesized true value). Given twenty-five sitters and a sample outcome of seventeen correct identifications (success rate of 68 percent) of their actual readings (which, given the discrete nature of the binomial distribution is the closest we can get to 66 percent correct) the one-tailed probability would be .054

and not less than .01 as Schwartz claims. Regardless of the correct probability value here, this has little to do with power. Schwartz is hypothesizing that the true (population) proportion of correct binary choices in this situation is close to the 67 percent (4 out of 6) that he observed in his sample. If, indeed, this value is correct, then, given his use of a one-tailed test and a significance level of .01, the probability of getting a significant outcome with twenty-five sitters would be slightly more than 0.54. To have a reasonable power (say close to 90 percent) one would need over 100 sitters.

Schwartz appears to be begging the question here. He begins by observing that four out of six sitters correctly identified which of two readings was meant for them. Because of the small sample, this outcome is consistent with a number of possibilities including the chance value of 50 percent. If he had obtained the same proportion of correct hits with a larger sample, then it would have been significant. However, since we cannot tell what the true proportion is from a sample outcome based on only six cases, we have no basis for predicting the outcome for a larger sample. His argument reduces to the trivial one: If the true proportion is 67 percent then we will be able to get a significant outcome with a larger sample. From his actual outcome, we can just as well say: If the true proportion is 50 percent (and this, too, is consistent with his data), then he will very likely *not* get a significant outcome with a larger sample.

I find it difficult to understand why Schwartz considers this point worthy of mention. Of course a binary outcome with only six trials has very low sensitivity. However, he did not rely on this outcome. He used two other measures, the number of dazzle shots and the hits and misses, which are clearly much more sensitive. These also failed to provide overall significance. For these measures (as well as for the actual choice of the relevant reading), the overall sensitivity would have been greatly enhanced if each sitter actually rated all six readings. In addition to greatly enhanced sensitivity, this would have avoided the unfortunate situation where each sitter was rating his or her own reading against a foil that differed for each rater. Another plus would have been the opportunity to determine which readings had more general appeal independent of any specific information peculiar to a given sitter.

In his longer rebuttal to my critique which he posted on the Web (see his reference in his rebuttal) Schwartz claims he actually predicted that GD would successfully differentiate his own reading from the accompanying foil reading. The claim that this particular outcome was predicted does not square with the opening sentence of the report wherein the experimenters state, "This paper reports an unanticipated replication and extension. . . ."

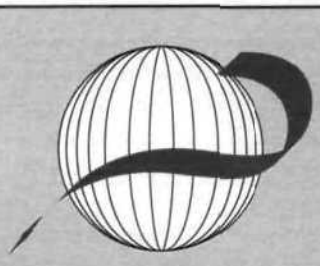
I have already pointed out in my critique how Schwartz has an unusually liberal interpretation of "replication." Not only is the statistical and experimental evidence suspect, but the qualitative analysis of the actual reading for GD in the second experiment does not overlap in any important respect with the reading in the earlier experiment. In particular, none of the apparently striking examples of names, events, and places that are reported for the first reading are in the second reading. I agree with Schwartz that the outcome of this "double blind" experiment is consistent with "individual differences in sitter characteristics." However, borrowing from Schwartz's propensity to resort to Occam's Razor, I believe it is prudent to suggest a much more mundane explanation. We need only assume two very plausible and non-extraordinary assumptions to account for the results: 1) Luck: GD had a 50-50 chance of choosing the correct reading; 2) Rater bias: given that he has chosen the correct reading, he would show a strong response bias to give high marks to the chosen reading and low marks to the rejected one. Note that this is consistent with the qualitative evidence that I provided in my critique. However, note that the burden of proof is not upon the critic to show that this explanation is correct. Rather, the burden of proof should be on Schwartz to show, as the claimant, that he has ruled out this and other possible mundane explanations. This is what good experimental methodology, which is so far lacking in the afterlife experiments, is intended to accomplish.

Unfortunately, I do not have space to respond to other specifics of Schwartz's rebuttal. In his rebuttal he attributes motives, preferences, and biases to me. These are based on assumption unsupported by facts. For example, he characterizes me as "reluctantly" agreeing that fraud is unlikely. In fact, I have no reluctance at all to make such an assertion. He attributes certain preferences to me that are, in some cases, just not true. He also is factually incorrect on some matters. He says that I was one of the group of cold readers who declared that I could, with training, duplicate what his mediums had accomplished in his laboratory. This is wrong. I deliberately refrained from such a commitment. My major point during the meeting with him on cold reading was that the determination of whether his mediums are using cold reading is a separate matter from the question of whether they were conveying any information of a paranormal nature. If he wanted to study the role of cold reading in the readings given by his mediums, that was an experimental goal that was separate from determining if his mediums are providing evidence for the survival of consciousness.

Nor did I conclude, contrary to Schwartz's implication, that his mediums were using cold reading. I did observe—and I specifically emphasized that this was a subjective opinion—that I could see little difference between the utterings of his mediums and those of the typical psychic reader. I want to emphasize again, it is not for me, or other critics, to show that his mediums are using cold reading or some other ploys. The burden of proof is on Schwartz to show that he has convincingly eliminated such possibilities.

So far as I can tell, Schwartz has really not answered my criticisms. A close reading reveals that he does not deny the various failings I have divulged in his research. Instead, he defends the departures from proper experimental methodology on a number of grounds: 1) he and his colleagues were aware of these defects and actually admitted so in their reports (but such admissions do not somehow neutralize the defects); 2) there were practical reasons such as wanting to provide a more naturalistic context (but this does not excuse using inappropriate control comparisons, failing to correct for rater bias, using inappropriate probability and statistical computations, etc.); 3) some of the "defects" were deliberately included to check on certain questions (but this does not justify drawing strong conclusions); and 4) that taken in their totality the experiments somehow provide powerful evidence for anomalous communication even if the individual experiments are flawed (actually, repeatedly making similar mistakes from experiment to experiment compounds rather than compensates for the errors).

Despite the deficiencies in his experiments, Schwartz seems convinced that his mediums have provided, in some cases, specific and unique information including names, places, etc., that the critics cannot explain away. For one thing, these apparently specific items are much fuzzier than he believes. His examples are selected just because they appeared to contain such specifics. This raises the difficult question of how to actually assess how much of this is just coincidence. Furthermore, even the most specific and concrete match is problematical because practically no constraints are placed upon the sitter in finding a suitable match (e.g., it can be a dead or a living person; it can be someone close to the sitter or a mere acquaintance; etc.). No actual check is made as to how close the match actually is. My point here is that Schwartz really has provided us with nothing to explain. We do not know if he has produced anything worth taking seriously until he can convincingly demonstrate that he has obtained his data under methodologically appropriate conditions. Science demands this in the conventional fields of inquiry. We should demand no less from Schwartz. □



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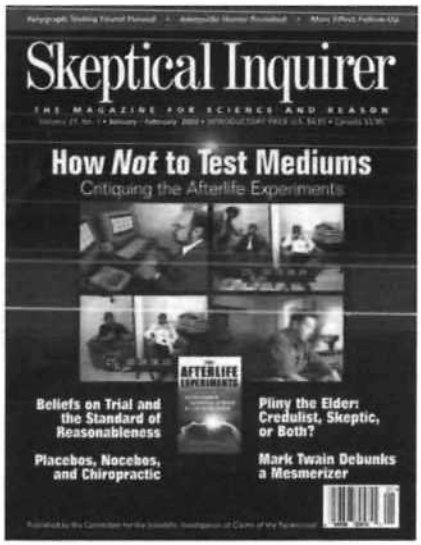
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Gary Schwartz's Strange Research at Arizona

In Ray Hyman's "How *Not* to Test Mediums" (January/February 2002) he states that "in August 2001, [Gary] Schwartz assembled a panel of seven experts on cold reading. . . ."

I could quibble on how Schwartz and/or Hyman would define "expert" in this context, but won't, as I was one of those assembled. And, based on that two-day experience, I thought I'd offer some comments on the article:

Ray Hyman and I have long shared a mutual respect for one another, but we do not always agree on various topics. This is one such case, as I feel Hyman was far too generous in assessing *The Afterlife Experiments.*

Gary Schwartz is an engaging, even charming fellow, but his understanding of cold reading is remarkably minimal (and largely mistaken), and it seems that he has little knowledge or interest in the fundamentals of protocol design and the interpretation of probability. Professor Schwartz is not a stupid man. However, given his seemingly willful ignorance combined with astonishing hubris, he might as well be.

Max Maven
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Missing from Ray Hyman's effective dissection of Gary Schwartz's past-life experiments was any explanation of how Schwartz, with all his academic credentials, careened into such outlandish views and such shabby thinking in the first place. Schwartz's work at the University of Arizona is hardly limited to "past-life" considerations; he has been deeply involved with the mystical alternative medicine projects of the University of Arizona's other shabby thinker Andrew Weil as well, serving as Senior Research Advisor to Weil's program.

One common denominator is Schwartz's and Weil's education at Harvard in the psychedelic 1960s—when chemical paths to "creativity" were all the rage. Weil, of course, can properly be called a psychedelic mystic; he admits he still uses LSD to tune his creative "natural mind" and recently has been the object of ridicule for his assertions that LSD is an effective treatment for cat allergies.

Has a psychedelic cult been established at the University of Arizona? Indeed, in 2001, psychiatrist Dr. Stanislav Grof, the leading guru of paranormal psychedelicism, delivered a Grand Rounds lecture to the Department of Psychiatry titled "Psychology of the Future: Lessons for Modern Consciousness Research." Grof then met with a research team purporting to be studying the use of psilocybin for treatment of Obsessive Compulsive Disorder. This questionable research is a project of the Multidisciplinary Association for Psychedelic Studies—a Groffian organization that endorses psychedelics to amplify paranormal powers. The University of Arizona's research team, as part of their "research" preparation, underwent Grof's hallucinogenic psychotherapy called Holotropic Breathwork!

Another product of 1960s-1970s psychedelicism, Dr. Lewis Mehl-Madrona, has just been added as Coordinator of Integrative Psychiatry to Andrew Weil's research team, funded by the National Center for Complementary and Alternative Medicine. Mehl-Madrona, a mystic with an extraordinarily checkered career, is also a product of the Grof-Esalen Institute circles. Known for his book *Coyote Medicine*, his medical practice might best be called "peyote" medicine. (See SI July/August 2000, "SRAM Articles Lead to Alternative Medical Doctor's Resignation.")

Mehl-Madrona and Schwartz will be sharing keynote responsibilities at an upcoming conference exploring "clinical practices integrating mind, body, and spirit" at Arizona State this coming February.

Gary Schwartz regularly shares mystical conference billings with Grof and his followers, as any Internet search can verify. My substantive study of Stanislav Grof's influence on alternative medicine, "Carl Jung, Stanislav Grof and New Age Medical Mysticism" has been published in the Spring 2002 issue of the *Scientific Review of Alternative Medicine.*

I would think that the public exposure of these psychedelic spiritualist networks is at least as effective a way to deal with obvious irrationalists like Schwartz as the meticulous point-by-point rebuttal of Ray Hyman. Schwartz, like all paranormalists, has an infinite capacity to spew out defective research. Is it not better to expose the very roots of the defective thinking itself rather than spend our own limited time constantly rebutting his nonsense?

E. Patrick Curry
Pittsburgh, Pennsylvania

Ray Hyman has exposed the flaws in Gary Schwartz's claim to demonstrate that mediums can communicate with the dead. Supported by public funds, Schwartz and his research methods also inform the academic "integrative medicine" movement. As a colleague of alternative medicine guru Andrew Weil at the University of Arizona, Schwartz contributes to claims that "conventional" medicine is preeminent only because it is "politically dominant," that the modern classification of diseases refers to "Western disorders," that modern physicians are unaware of emergent properties, that they are unlikely to be open to "new paradigms," and that they do not "emphasize healing the person as a whole" or understand that "a given disease may manifest at the spiritual level as well as on the physical plane." As an antidote to such failings, Schwartz, Weil, and their colleagues offer traditional Chinese Medicine, Ayurvedic Medicine, Native American Medicine, and homeopathy.

Since these disciplines are not readily amenable to study by "Western reductionist" methods, Schwartz and Weil argued (in the January 28, 2002, *Archives of Internal Medicine*), they must be studied in a "systems theory framework." A standardized acupuncture regimen oughtn't be studied as a treatment for patients with asthma, for example. Rather, patients with the "Western" diagnosis of asthma should be individually diagnosed and treated by practitioners of Traditional Chinese Medicine, using whatever diagnostic and therapeutic options they deem appropriate—however numerous and disparate these may be. Outcomes must not be reductionist either: in addition to "disease-specific conventional outcomes" there should also be "psychological, social, and spiritual outcomes in the same study."

The only trouble, the authors grant, is that these schemes will introduce so many variables that it will be impossible to draw any conclusions. No problem: "given many choices, the patient's preferences for the course and sequencing of action guide the ultimate decision-making process." Likewise, the difficulty in conducting randomized, controlled trials under such conditions isn't a problem because "not only are observational studies justifiable under certain conditions, they may even have several advantages over randomized controlled trials, including lower cost, greater timeliness, and a broader range of patients."

Schwartz is the recipient of a grant from the NIH's National Center for Complementary and Alternative Medicine for his "Center for Frontier Medicine in Biofield Science." According to the NCCAM Web site, "This Center facilitates and integrates research on the effects of low energy fields. The research is focused on developing standardized bioassays (cellular biology) and psychophysiological and biophysical markers of biofield effects, and on the application of the markers developed to measure outcomes in the recovery of surgical patients.

As unlikely as it may seem, some influential academic physicians take such ideas seriously. Ralph Snyderman is the Chancellor for Health Affairs at Duke University and President and CEO of Duke University Health System. He has "fostered discussion of Integrative Medicine at meetings of the Association of Academic Medical Colleges," of which he is the chairman. In a recent article for *Academic Medicine*, Snyderman and former Schwartz and Weil co-author Tracy Gaudet praised Weil's "Arizona Program" and called for incorporating "integrative medicine" into the standard medical school curriculum. Elsewhere they have written, "We believe that what we now call integrative medicine will one day be such an integral part of medical education, training, and practice that it becomes the new paradigm for good conventional medicine."

Kimball C. Atwood IV, M.D.
Waben, Massachusetts

(See also the Follow-Up section in this issue, pp. 58–64 —EDITOR)

'Reasonable Person' Standard?

If Richard M. Fisher's purpose ("Beliefs on Trial and the Legality of Reasonableness," January/February 2003) was to justify the application of litigation paradigms to the skeptical inquiry into claims of the paranormal, he has failed to do so, and accordingly the jury should be directed to dismiss his case!

The pith and substance of his argument seems to be that the standard of proof of paranormal phenomena should be one that would satisfy ordinary people sitting on a jury. This standard depends on "what is reasonable in the circumstances," which in turn depends on what would be expected from persons standing "in the shoes of the claimant."

In this way Fisher seeks to cram the "Reasonable Person" (RP) standard into a context in which it was never intended to operate. Originally formulated by the courts as an objective standard, the RP becomes subjective: it is to be defined in relation to the specific "education, experience, and capacity" of the person making the paranormal claim. Next he changes the RP concept from a standard for assessing a defendant's past conduct into a yardstick for measuring the credibility (or fallibility or gullibility) of a witness—something the law does not do.

This leads to Fisher's extraordinary suggestion that a claim by an air traffic controller to have seen a UFO is to be assessed according to whether "a reasonable air traffic controller, with similar background and experience to the claimant" would have been led to the same inescapable conclusion. I'm sorry, but the answer to that question proves nothing to me about the existence of UFOs. Would Fisher be satisfied with an alien abduction story told by a country bumpkin, provided it's good enough to persuade a jury of "reasonable" country bumpkins?

Fisher disdains the evidence of experts, because not all experts are skeptics, and some are "out-and-out flakes." We don't need experts, says he; all we need is the Reasonable Person. This populist, anti-intellectual viewpoint may be in keeping with the current zeitgeist, but I still like to think there's room for scientific evidence to be brought to bear on claims of the paranormal.

Fisher doesn't like the idea of requiring extraordinary proof of extraordinary claims, unless it's "reasonable in the circumstances" to require it. Reasonable people can differ over what's reasonable, so where does that leave us? The standard, he says, depends on what would be expected from any other person with similar qualifications and experience to the claimant. Again, a subjective test for proving objective facts.

Jeff White
Toronto, Ontario

I frequently encounter misapplications of legal reasoning other than those described by Richard Fisher. A common example is invoking a variation of "innocent until proven guilty," for example, UFO sightings are real until proven wrong. This ploy, of course, effectively places the burden of disproof on science. Criminal defendants are innocent *until proven guilty, but ideas are wrong until* proven right.

A much better model is the civil standard of "preponderance of the evidence." If the vast majority of UFO sightings are erroneous, the preponderance of the evidence suggests that the remaining unexplained cases are, too. Even more useful is the doctrine of "control of the facts." The burden of proof is frequently on the side that was best in a position to record the facts. That's why taxpayers are required to keep financial records. Similarly, both the orthodox scientist and the believer in UFOs have the responsibility for collecting convincing data and ensuring that it meets stringent standards.

In the ongoing debate about the Mars Effect (the Ertel-Dean exchange), there is one possible explanation I have never seen discussed. If you scan a large body of data for apparent correlations, you will probably find a few. If you focus solely on the apparent correlation and ignore all the other possible combinations and permutations, the correlation will stand up to every imaginable statistical test. No amount of post-hoc statistical analysis will make a run of ten consecutive heads in coin flipping go away, but if we find out the ten heads are a selected part of a series of a thousand flips, the run becomes a *lot less remarkable. If the Mars Effect is* nothing more than statistical clustering, no amount of post-hoc statistical testing of the original data set will discredit it. To test whether the Mars Effect is real, we need to look in a broader context. Are there equally strong analogous correlations between other planets and other traits? No. Does the effect persist when examined over a longer time span than the original data? No. The preponderance of the evidence is that the effect is statistical clustering, nothing more.

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Green Bay, Wisconsin

When a skeptic undertakes, as does Richard M. Fisher, to pontificate on the need to apply proper scientific and statistical

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methodology, a useful first step for the skeptic is to acquaint himself with the basic principles of science and statistics. In the article, Fisher makes the following statements: "For publishing in most scientific journals, the minimum standard is P<.05. In other words, in order to convince me, and others, that the results are valid, you must first be able to show, by statistical analysis, that if you repeated the experiment 100 times, you would be expected to get the same result 95 times out of 100."

What Fisher seems not to realize is that the ".05" in the phrase ".05 level of statistical significance" refers to the probability of *what is called a Type I error (obtaining a* significant result if the null hypothesis is true). Thus, a correct version of Fisher's statement would be something like: "If only chance is operating, and you repeated the experiment 100 times, you would fail to get a significant effect 95 times out of a 100."

Fisher has instead erroneously assumed that the .05 refers to what is called a Type II error (failing to get a significant result when the null hypothesis is false). The probabilities of making a Type I and Type II error are not in general the same. For instance, suppose that a weight-loss program really does produce a loss in the average weight of its participants. If the effectiveness of the program is tested using a .05 level of significance, the probability of a Type II error (getting a nonsignificant result) is likely to be much greater if the program produces an average weight loss of three ounces than if it produces an average loss of twenty-five pounds.

Fisher's error would be sufficient to earn him a B- in Statistics 101 (any lower grade being of course precluded by contemporary grade inflation and the ever-present possibility of litigation).

Douglas M. Stokes
Ann Arbor, Michigan

Causes and Precognitions

Massimo Pigliucci's new column is a wonderful idea, and he chose an excellent topic for his first entry (January/February 2003). Unfortunately, in conveying the essence of the post hoc fallacy, he used a terrible example which severely undermines the impact of his article.

When people claim to have had precognitive dreams, they *do not,* as Pigliucci says, imply "some causal connection between the dream and the phone call." They merely believe that they have had advance warning *of something that was going to happen.*

If I had fixed the World Series, for example, and then telephoned a crony on the opposite coast to tell him about it, that would not imply that the call caused the fix.

John Orr
Fullerton, California

Askance at Yogic 'Miracles'

I greatly enjoyed Massimo Polidoro's "Don't Try This At Home" (January/February 2003). To his suggestions for further reading, I would like to add Arthur Koestler's delightful travelogue and critique of both India and Japan, *The Lotus and The Robot* (Hutchinson 1960), which includes a description of various bizarre and, in Koestler's opinion, often "degrading" Yogic practices. Koestler concludes that yogis can achieve high levels of conscious control over various neuroendocrine functions which are normally autonomic, and can, for example, raise boils, stigmata, etc. through meditative exercises.

In no case, however, could he find evidence of a genuine miracle, one in which a law of physics as we know it was clearly violated by a Yogic feat. Koestler chose levitation as an example; no yogi has ever been reported to be able to alter his own weight by even a small amount, when measured under controlled conditions.

Koestler and others have noted that, when Vedic philosophy is exported to the West, either as New Age stuff (and nonsense) or in the older writings of Aldous Huxley, Christopher Isherwood, etc., the role of the miraculous is usually downplayed. In India, however, the story was very different in 1960, when Koestler wrote, and probably remains so today in many areas of the country.

A typical New Age/spiritualist response to a skeptic's demand for proof is that yes, Yogic miracles are possible, but only at great risk to the practitioner, and that miracles are in any case not for public consumption. Nonsense! Jesus' "miracles" were allegedly performed for the multitudes, not just for "initiates," and included wine, transformed from water, enjoyed by unbelievers. If "advanced" yogis can in fact perform miracles, there is no necessary "karmic law" that would prevent them from doing so under laboratory conditions. One very strongly suspects that yogis do not give such demonstrations because they are in fact unable, rather than merely unwilling, to do so. If so, the tacit encouragement of public belief in *the possession of miraculous powers by various* "gurus" gives one good cause to seriously question the integrity and honesty of these "spiritual leaders," and, by extension, to look askance at the fundamental precepts of their tradition.

John A. Finch
Madison, Wisconsin

Letters on 9/11 Concerns

A more rational, objective alternative to the events of September 11, 2001, (Letters, January/February 2003) could unwittingly provide a framework for minimizing the human significance of an act of profound moral evil. A rational analysis that objectifies the personal and symbolic meanings of this act of terrorism can easily "spill over" into a rationalization that minimizes the meaning of this evil act and more important, leads to a neglect of prophylactic action and healing.

A purely logical analysis of the terrorist attacks is characterized by both strengths and weaknesses. Strengths include an analytical, objective examination and an emotional distancing from the event. Weaknesses include the objectification of an event that should be, at least in part, experienced within an emotionally empathic framework of shared meanings.

The "disproportionate" response is not at all disproportionate when viewed in psychological terms. The meaning of this event should be recognized in terms of implications for such core American identity elements and values (arguably somewhat illusory) as security and safety. The core identities of many Americans were threatened—exposing millions to vivid awareness of the fragility and contingency of human life. The symbolic meaning of this event cannot be experienced within the context of a purely rational analysis, but must be examined from multiple perspectives, including subjective, emotional, symbolic, and collective meanings.

Jerome J. Tobacyk
Psychology Department
Louisiana Tech University
Ruston, Louisiana

The central point to be made about the response to 9/11 is not whether it was disproportionate but rather whether any effective

response can be made, other than the exercise of better than usual care (i.e., "vigilance").

Although it wasn't the central point of their letters, respondents Thomas Freeman and Charles Chandler touched on it. Chandler wrote, "The next big terrorist attack will probably have nothing to do with air transport," and Freeman added, "My engineer friends and I spent time at lunch dreaming up terrorist scenarios." To see how easy it is to come up with horrific acts against which virtually no defense is possible, especially when the perpetrator is willing to commit suicide in the process, imagine several terrorists, say, hijacking several gasoline tank trucks one day and driving them into, say, several banks around the country. This would require little in the way of planning and almost nothing in the way of equipment. I will probably be accused of encouraging terrorism for giving this example, but surely it is clear this isn't rocket science. How many mailboxes did just one mixed-up teenager blow up before he was caught?

So Chapman and Harris's comparison with other risks is the only comparison that matters. Reinforcing cockpit doors is a good (and overdue before 9/11) idea. Catching bin Laden would be a great (and overdue before 9/11) event.

But I don't think we now need a Federal Bank Petroleum Truck Approach Radar and Barricade Network or a National Teenage Mailbox Antipathy Screening Network.

Jonathan R. Smith
Brunswick, Georgia

Kudos for a Professor

Yesterday, I finished my last day of class for Computer Science 540: Introduction to Artificial Intelligence. The course was taught by Professor Jude Shavlik at the University of Wisconsin-Madison. I believe he deserves an honorable mention in SKEPTICAL INQUIRER for two instances I observed during the course of the semester:

First, the class watched an ABC *Nightline* episode titled, "Machines Like Us." The final part we watched talked about an investment business using genetic algorithms to create artificial stock pickers. *Nightline* focused on the successes of the featured business. Professor Shavlik informed us that a former graduate of UW-Madison had worked for the company, and ended that day of class by saying, "Well, he no longer works for the company, and he's not independently wealthy. So perhaps let that be the final lesson, that maybe in a bull market lots of things will work that might otherwise not."

Second, on the last day of class, we briefly reviewed some of the philosophical topics related to artificial intelligence. In the closing remarks for the course, he asked the students to "be skeptical of claims about artificial intelligence. Don't let some snake oil salesman convince you of some machine learning technology because 'it's got genetic algorithms' or 'it's based on nature.'"

In summary, Professor Shavlik was not only an excellent professor who taught CS 540 very well, but also a proponent of skeptical thinking in the field. For this, I feel he deserves an honorable mention as one of the best professors I've encountered in my undergraduate career. Professor Shavlik's home page is www.cs.wisc.edu/~shavlik/.

Patrick Meade
Madison, Wisconsin

The Report of Our Extinction May Be Exaggerated

Richard M. Fisher, reviewing *The Skeptical Environmentalist: Measuring the Real State of the World* by Bjorn Lomborg (November/December 2002) takes its author to task for resurrecting a statistical straw man: "he quotes from the work of a scientist who stated in 1979 that we could be losing something in the order of 40,000 species a year to human-driven extinction. To increase the caricature, Lomborg reprints the 40,000 species number on an X-Y graph so that the 40,000 figure spikes up like a sore thumb from a near-zero baseline from the years 1600–2000. Lomborg states that this 'is a figure which with monotonous regularity has been repeated everywhere until in the end we all believed it.' The problem is, it hasn't, and we don't."

This will come as a surprise to the millions who have seen just such a graph in a book that is very much still in print. It is not a product of the 1970s, but Al Gore's best seller *The Earth In the Balance*. Its depiction of the exponential rate of species loss is literally impossible to exaggerate, since its curve goes vertical, and hence its rate of extinction infinite, upon reaching the year 2000.

Despite having been extinct for these last three years, I nevertheless look forward to Richard Fisher's review of the newest and most elegant edition of this Vice-Presidential masterpiece. Perhaps he will commend it to The Danish Committees on Scientific Dishonesty.

Russell Seitz
Nantucket, Massachusetts

ID and NEA

This note relates to the concept of Intelligent Design creationism, and specifically to the "Unintelligent Design" Forum article in the November/December 2002 issue of SI.

I am a career public school teacher, and I'm saddened to learn that the National Educational Association (NEA) of which I'm a member apparently is going to provide a forum for ID proponents. This note appears in the January 2003 issue of *NEA Today*, distributed to all members:

"Should Schools Teach 'Intelligent Design'? If you'd like to take part in this future debate, send a brief note to Alain Jehlen, ajehlen@nea.org."

This is a yes/no "Debate" page, a regular feature of the educational journal.

Brant Abrahamson
Brookfield, Illinois

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Dumbbell Nebula

An aging star's last hurrah is creating a flurry of glowing knots of gas that appear to be streaking through space in this close-up image of the Dumbbell Nebula, taken with NASA's Hubble Space Telescope. The Dumbbell, a nearby planetary nebula residing more than 1,200 light-years away, is the result of an old star that has shed its outer layers in a unique display of color.

Image Credit: NASA and the Hubble Heritage Team (STScI/AURA)
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